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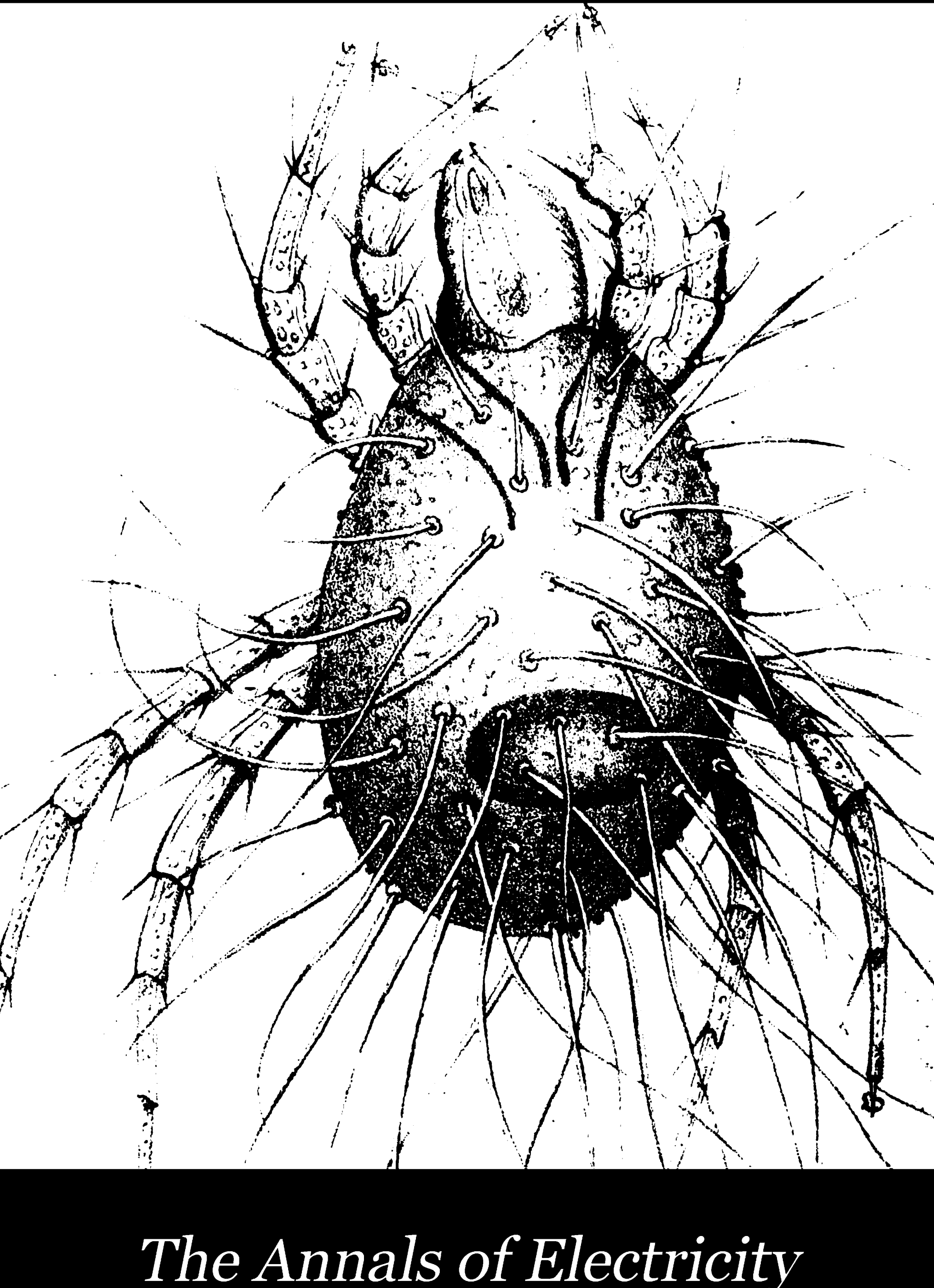

The Annals of Electricity Magnetism and Chemistry ...

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THE ANNALS

OF

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AND

Guardian of Experimental Science.

CONDUCTED BY

WILLIAM STURGEON,

Lecturer on Experimental Philosophy, at the Honourable East India Company's
Military Seminary, Addiscombe, &c. &c.

AND

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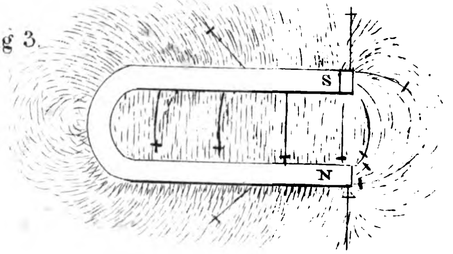
Fig 3.



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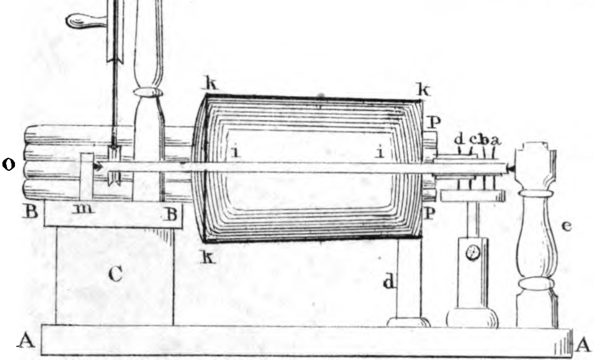

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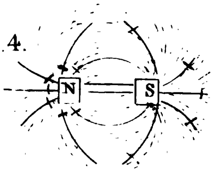

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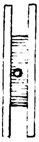

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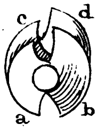

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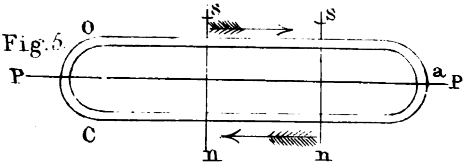

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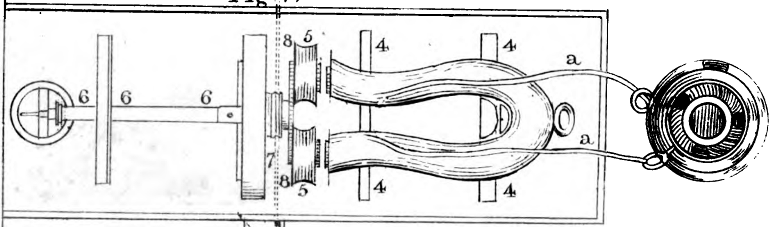

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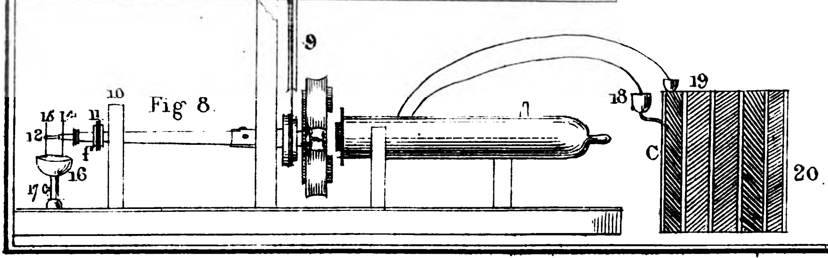

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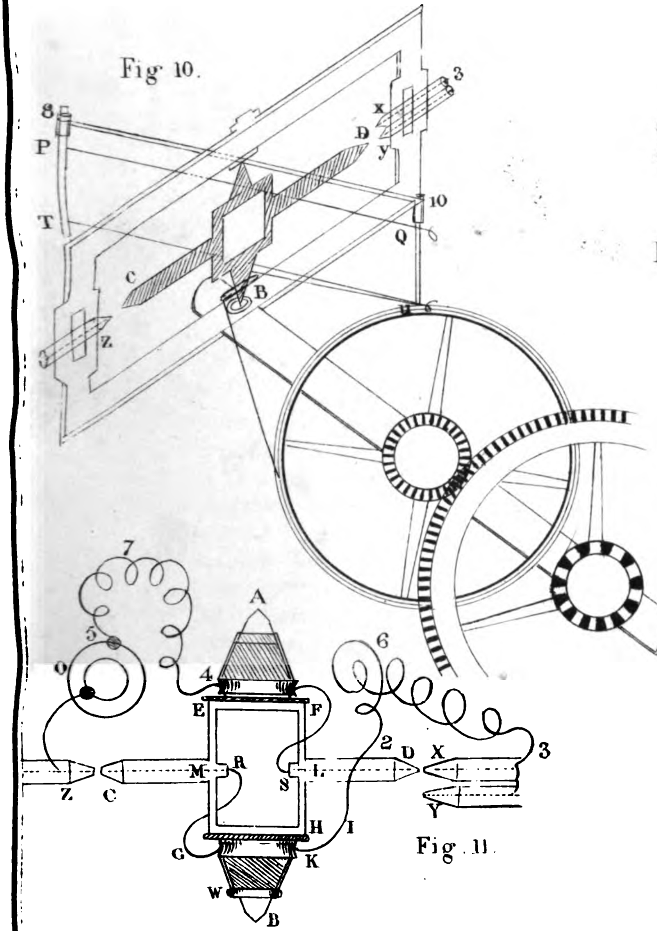
Fig 10.

Fig. 11.

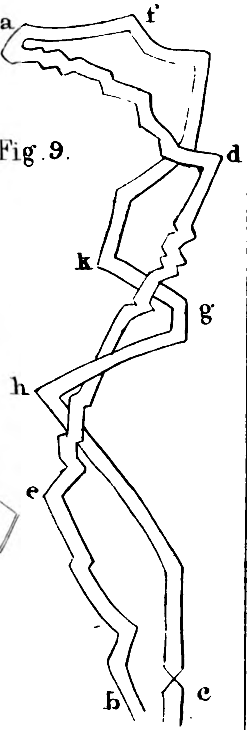
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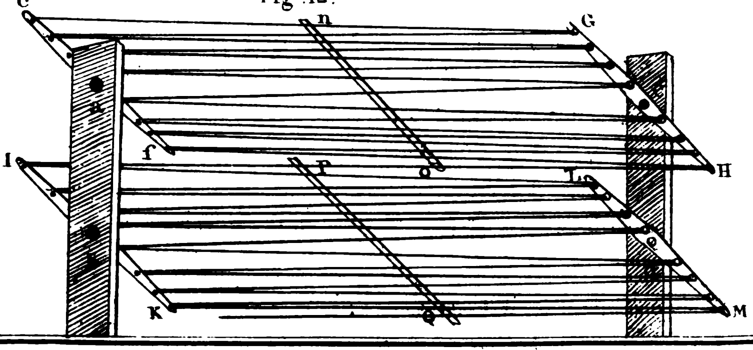
Fig. 12.



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THE ANNALS
OF
*ELECTRICITY, MAGNETISM,
AND CHEMISTRY;*
AND
Guardian of Experimental Science.

JANUARY, 1838.

I. *Researches in electro-dynamics, experimental and theoretical, by* WILLIAM STURGEON, *Lecturer at the Hon. East India Company's Military Academy, Addiscombe.*[*]

Perhaps there is no branch of experimental enquiry, at the present day, more interesting than that of electro-dynamics; nor has any department of science been more successfully pursued, since the commencement of the present century.

The two leading classes of phenomena exhibited by electric currents, are the chemical and magnetical; both of which have been discovered within this period, and have become the most important established divisions in the study of electricity.

The rapid, and unprecedented series of successful enquiries which led to the establishment of these important branches of electricity, had their *origin* in the invention of, and their happy progress *dependent* upon, the voltaic apparatus; the novel and potent energies of which developed these beautifully interesting and unexplored fields of scientific research.

Notwithstanding, however, the unquestionable supremacy of the voltaic battery, in the production of electric currents, and the splendid discoveries which have been accomplished by its employment, it must ever be acknowledged that, even in its most improved forms, it is a troublesome and expensive apparatus in the process of experiment: and the continual and unavoidable diminishing of its powers, whilst in action, is a defect, whose remedy is necessarily precluded by the destructive process required for its excitation.[†]

---

[*] This paper is a part of one which was read before the Royal Society, June 16th, 1836, but not printed in their Transactions.

[†] At the time this paper was written, I was not aware of Professor Daniell's improvements in the voltaic battery. But even now, I can see no reason to change my opinion regarding the expense and nuisance attending the employment of voltaic batteries; nor do I despair of magnetic electrical machines being brought into general use as implements of experimental research.

The discovery of magnetic electricity, however, has led to the construction of novel apparatus, capable of producing continuous electric currents, of undiminished energy, for any length of time they may be required to be in operation; and, at the same time, free from all those defects of the voltaic battery, and the objections to its employment, with which it must ever be attended.

The magnetic electrical machine, whose exhaustless powers, free of expense and ever ready at command, when brought even to a moderate state of perfection, can hardly fail of becoming a powerful engine of analysis, and an useful and economical implement in the hands of the experimental enquirer. In its present state, its powers rival those of moderately sized batteries; and it is highly probable, that they may be so far exalted by future improvement in the machine, as eventually to supersede the voltaic apparatus in this branch of physical investigation.

No description, that I am aware of, of apparatus of this kind, which can properly be called an implement of investigation, has yet found a place in the scientific journals of this or any other country; but, considering the probability of their becoming highly advantageous to the experimental philosopher, when their energies shall have become properly represented, and duly appreciated: and in order to call the attention of those who are the most likely to form a proper estimate of these machines, and to be benefited by their introduction to experimental research, I have ventured to offer to the notice of the Royal Society, a description of two of those forms which I have given to them; and also a brief detail of a few experiments which I have made with them, and which may be considered as expressive of their respective powers. I have also, by way of comparison, detailed a few experiments made with a voltaic battery, and have ventured to draw a few of the most obvious conclusions with regard to the respective powers of this apparatus and the magnetic electrical machine; and of the advantages likely to be derived from the improvement of the latter.

Description of a Magnetic Electrical Machine, having no iron armature.

In fig. 1, Plate I, which is a longitudinal section of the apparatus, A, A, is the edge of a rectangular mahogany base board, about eighteen inches long from A to A, and ten inches broad; its thickness one inch. B, B, is the end of another mahogany board, ten inches long, from front to rear, and four inches broad, from B to B. It is supported over one end of the base board by two square pieces, one of which is seen at C. The cross board B, B, thus elevated above the base, forms a stage for the support of the bend O, of a horse-shoe magnet O P P, the poles being supported by two pillars, near to the opposite edges of the base board. A part of one of these pillars is seen at *d*. Near the summit of each pillar is a notch with an outside shoulder to prevent the magnet from slipping sideways. A metallic stud *m*, rises above the stage and directly over the axis of the base board. This stud carries a steel pivot, on which runs one extremity of the metallic spindle *i*, *i*. The other extremity of this spindle also runs on a centre pivot, which projects at right angles from the pillar *e*, the latter being fixed to the base board.

The spindle *i*, *i*, passes through the pillar *f*, (which supports the wheel *g*,) and also through the axis of the reel *k*, *k*, *k*, *k*, to which it is fixed. On the reel is coiled two hundred feet of copper wire, about 1-20th of an inch diameter, and covered with stout white sewing silk, to prevent metallic contact in the coil. The spindle, with its reel and coil, are put into rotatory motion by means of the wheel *g*, and a band which passes over the pulley *h*. The reel which holds the wire is made of two thin pieces of deal, of the shape *k*, *k*, *k*, *k*, which form the cheeks, and are kept at about one inch and a quarter apart, and parallel to each other, by two pieces which cross them in such a manner as to leave a deep groove all round between them, for the reception of the wire. An end view of the reel is seen in fig. 2. The extremities of the wire, forming the coil, terminate in a discharging arrangement to be described in the sequel.

On the stage B, B, and pillars *d*, *d*, is placed a compound horse-shoe magnet, composed of four bars of steel, which together weigh about 23 pounds. The poles of the magnet are five inches apart, and near to the bend the branches are about six inches apart. The length of the magnet, inside, is about eleven inches: the breadth of each bar about an inch and a half.

When the magnet is placed on its stage, its plane is parallel to the plane of the base board. The spindle *i*, *i*, which is also parallel to the axis of the base board, is situated in the axis of the magnet. By this arrangement, the coil is made to revolve between the branches of the magnet, and electric currents are excited whilst the coil travels through the magnetic lines, according to the laws of magnetic electricity.* The *direction* and energy of the currents will depend upon several circumstances which it will now be necessary to explain.

---

* See Vol. I. p. 251 to 260 of these Annals.

Excitation of the electric fluid in the coil.

In describing the electric excitation whilst the coil performs its revolutions between the branches of the magnet, it will be necessary to take into consideration the position and direction of the magnetic lines of force through which it has to travel, and which give the exciting impressions. This, fortunately, is exceedingly simple, and requires but little attention to be understood.

If iron filings be strewed on paper, below which is placed a horse-shoe magnet, whose plane is parallel to the horizon, they will be arranged, by the magnetic force, similar to the arrangement of the fine lines about the magnet in fig. 3, which may be taken as a pretty exact resemblance of the position of the magnetic force in the plane of the magnet. And if we assume the marked end of the magnet, in the figure, to correspond with that pole of the compass needle which naturally is directed towards the north, and the unmarked end to correspond with the needle's pole solicited by the south, the strong black lines with cross heads will indicate a similar arrangement of polarity in the iron filings, or in any small pieces of iron situated between the branches of the magnet.

The position of the magnetic lines, between the branches, appears, by this arrangement, to be in planes parallel to the plane of the magnet; and at right angles to its axis.

A pretty exact representation of the position and direction of those parts of the magnetic force which lie directly above and below the space between the branches, will be obtained by strewing iron filings on paper, below which is placed the poles of a vertical magnet. Fig. 4 will serve to give some idea of such an arrangement, the strong curved lines with cross heads indicating, as before, the polar arrangement of the ferruginous particles. This figure serves to show that the magnetic force above and below the space between the branches of the steel, is exerted in curve lines, and in planes perpendicular to the axis and plane of the magnet.

These are the only parts of the magnetic lines of force through which the coil will have to travel, and, consequently, those only which will be materially concerned in the excitation.

Let *n s*, *n s*, fig. 5, denote the direction of the magnetic force between the branches of a horse-shoe magnet, and let *c o a* be an oblong ring of copper wire, also situated between the branches of the magnet, and susceptible of rotatory motion upon a spindle *p p* coincident with the axis of the magnet. If now the wire be made to rotate in such a manner that the side *o a* moves downwards, and consequently the side *c a* upwards, those sides will pass through, and at right angles to, the magnetic lines; and the electric current thus produced, will rush through the wire ring in the direction of the arrow. But as the wire proceeds in its revolution, the angle between the direction of its motion, and that of the magnetic lines through which it travels, will become less and less, (see fig. 4); on which account, as also in consequence of the diminution of the force, above and below the magnet, the excitation will gradually diminish. When the plane of the copper ring has proceeded through a quarter of a circle from its first position, its plane will be at right angles to the plane of the magnet, and its motion in the direction of the magnetic lines. The excitation will then be at a minimum or zero.

As the wire moves on, the excitation recommences, but the current is reversed in the ring: because the part *o a*, of the wire, which moved downwards through the first quadrant, and pressed its foremost surface against the magnetic lines, will now move upwards, and receive the exciting impressions on the opposite side. A similar change in these particulars will take place in the part *c a* of the wire ring.

Moreover, as the wire proceeds through the second quadrant, the direction of the magnetic lines, and that of the revolving ring, will form a progressively increasing angle, which will become a maximum as the quadrant is being completed. On this account, and also in consequence of the increase of magnetic force, the excitation will be progressively exalted, whilst the plane of the ring is describing the second quadrant; and will be a maximum at the terminal point; or when the plane of the ring and that of the magnet are again coincident.

Whilst the ring revolves through the third quadrant, the circumstances connected with the excitation will be similar to those whilst travelling through the first. The excitation progressively diminishes, and becomes a minimum when the quadrant is completed: at which time the plane of the ring will be at right angles to the plane of the magnet.

When the ring enters the fourth quadrant of revolution, the excitation again recommences; but the direction of the current will be reversed, for the same reason that it was reversed when the ring entered the second quadrant. The excitation will be progressively exalted, until the plane of the ring be again coincident with the plane of the magnet; at which time the excitation will again be at a maximum.

It is now obvious that, during one entire revolution of the ring, there will be a series of vicissitudes in the degree of excitation, and also in direction of the current; and a similar series of vicissitudes will attend every succeeding revolution. Twice there will be a maximum, and twice a minimum of excitation; the former taking place when the plane of the ring is coincident with, and the latter when it is at right angles to, the plane of the magnet. The latter position of the ring is that in which the current changes its direction; and may very commodiously be called the *neutral plane.* The plane of the magnet is obviously the *plane of greatest excitation.*

If the revolution of the ring be permitted to commence at the *neutral plane,* the current will not change its direction until the ring has arrived at that plane again, or until it has passed through half a revolution: but there will be vicissitudes of energy in that current. The excitation will increase through the first quadrant, but will decrease through the second. And similar vicissitudes of energy will transpire, with regard to the reverse current, which will be excited during the progress of the ring through the other half of the circle of revolution.

If now, instead of a single ring, we had an endless coil of wire, revolving between the branches of the magnet, every convolution would receive exciting impressions as decidedly as the single ring; and currents, thus produced, would traverse the whole length of the wire forming the coil; and undergo all the vicissitudes of energy and direction, which have been particularized with respect to the ring.

Hitherto it has been supposed that the ring and the coil form, each a complete circle within itself, and consequently, the range of the currents limited to those circles; a mode which has been adopted merely to simplify the description of the vicissitudes which the excited currents will undergo during each revolution. But in the construction of the machine fig. 1, the ends of the wire, forming the coil, are not soldered together; but are attached to an arrangement of semi-wheels, properly disposed to discharge the excited currents in one and the same direction, through any apparatus which the experimenter may wish to place in the electric circuit. This discharging arrangement is seen at *a, b, c, d,* in figures 1 and 6, but will be better understood by describing the latter.

In fig. 6, which is a bird's-eye view over the end of the spindle, are four semi-wheels *a, b, c, d,* two of which, *a* and *b,* are soldered to a metallic tube, which passes through their centres: and the other two, *c* and *d,* are soldered to another tube, something wider than the former, which also passes through their centres.

The semi-wheels are placed at about a quarter of an inch from each other on their respective tubes, the smaller of which exactly fits the revolving spindle *i, i,* fig. 1; and the larger being lined with an ivory tube for insulation, is also made to fit the spindle, which, passing through both tubes, carries the four semi-wheels, which are, by this means, caused to make corresponding revolutions with the coil.

The longitudinal opening between the terminal points of the semi-wheels, as seen above and below the spindle *s* in fig. 6, is in the plane of the coil produced; so that the transfer of the current from one pair of semi-wheels to another may always take place at the neutral plane.

One extremity of the wire, forming the coil, is soldered to the tube carrying the semi-wheels *a* and *b*, and the other extremity to the tube carrying *c* and *d*. The semi-wheels revolve in a glass trough, supported on a pedestal *t*, as seen in fig. 1. The trough is divided into three compartments, in which is placed a sufficient quantity of mercury for the periphery of the semi-wheels to run in when on the lower side of the circle of revolution.

The semi-wheels *c* and *b*, run in the centre compartment, and *a* and *d*, in the outer compartments, which latter, however, being connected by a copper staple, the mercury placed in them may be regarded as belonging to the same metallic mass.

If now, the spindle, with its coil and appendages, be made to revolve in the direction indicated by the arrow fig. 6, the semi-wheels *a* and *c*, which are in connexion with the different ends of the coil of wire, would enter the polar cells of mercury; and would, (if the circle were completed by a wire, or any piece of apparatus, connecting those cells,) convey the electric current from one to the other: the direction of the current depending upon the connexions previously made between the extremities of the wire of the coil and the tubes to which the semi-wheels are attached.

When the spindle had made half a revolution, the order and position of the semi-wheels would be reversed; *a* and *c* would then leave the mercury, and *d* and *b*, would succeed them in their respective cells. At this time the coil would be in the neutral plane, and as it moved on, the current which traversed it, would flow in the opposite direction to the former. But *b*, which succeeds *c*, in the central portion of mercury, is soldered to the other extremity of the wire; and *d*, which succeeds *a*, is also soldered to the opposite extremity of the wire. Hence, it is obvious, by this arrangement, that the changes which take place in the direction of the current in the coil, are accompanied by corresponding changes in the connexions between the extremities of the wire and the mercurial cells. If therefore, whilst the coil travels through half a revolution, from the *neutral plane* to that plane again, the flow of the current *from* the coil be towards the semi-wheel *c*; the reverse current, generated during the other half revolution of the coil, would flow towards *b*: and as *c* and *b* succeed each other in the same cell, the mercury there placed would receive both currents from the coil.

A wire or any other conductor joining this mercury, and that placed in either of the outer cells, would convey both currents to the semi-wheels *a* and *d*, which, being attached to the opposite ends of the wire, would, in their turn, dispense the fluid again to the reciprocating currents in the coil. The centre cell which unites the reciprocating currents from the coil, on the one hand, may, for convenience, be called one of the poles of the apparatus: and the outer cells, which are connected, and unite the currents on the other hand, may be called the other pole.

Apparatus placed in proper connexion with these polar cells, would transmit the *united currents*, from pole to pole, in one uniform direction; whilst the revolutions of the coil were performed in one and the same direction. If the revolutions of the coil were performed in the opposite direction to the former, the electrical functions of the polar cells would be reversed; and the current through the apparatus necessarily reversed also.

EXPERIMENTS.

Magnetic.—A galvanometer, whose coil consists of eighteen feet of copper wire, one twentieth of an inch diameter, covered with sewing silk, and formed into eighteen convolutions, was placed in the circuit of the machine, by proper connexions with the polar cells. The magnetic needle belonging to the galvanometer is four inches long, and weighs 110 grains. It is furnished with an agate cap, and supported on a finely pointed steel pivot in the plane of the coil; and, at the time it was employed in these experiments, it had a directive force which caused it to vibrate ten times in thirty seconds; or it performed, at a mean rate, one vibration in three seconds.

The coil of the machine was made to revolve with three different velocities, viz. with three revolutions per second; six revolutions per second; and twelve revolutions per second; and with each speed in both directions. The needle's deflection due to the influence of one current in each case, being well ascertained before the coil was revolved the other way. The deflection due to the reverse current was also ascertained, and the mean of the two taken as the standard deflection, for each rate of motion of the coil.

The diameter of the wheel *g*, fig. 1, being twelve times that of the pulley *h*, the latter, and consequently the coil, revolves with twelve times the angular velocity of the former.

The speed of the wheel was measured by the seconds' pendulum of an Attwood's machine, which was found very convenient because of its addressing its motions both to the eye and the ear. The deflections of the needle, by this means, can be accurately observed, whilst at the same time the motion of the wheel can be nicely accommodated to the beats of the pendulum, without the aid of an assistant.

The results of the experiments are arranged in the following table.

Coil revolved to the	Deflections of the Magnetic Needle due to		
	Three Revolutions per Second.	Six Revolutions per Second.	Twelve Revolutions per Second.
Right.	27°	35°	44°
Left.	23°	30°	40°
Mean.	25°	32·5°	42°

The arcs of deflection exhibited by the above table, are those marked by the needle after it had ceased to oscillate, and had become perfectly stationary; and at which it could be kept steady for any length of time required, by a due and uniform motion of the revolving coil.

The above results present the two following remarkable circumstances. First, there is an obvious increase of power by an increase of velocity, but does not appear to be any particular accordance between the velocity of the coil and the arc of the needle's deflection: the ratio of the one being very different to that of the other. Neither are the tangents of those arcs proportional to the velocities.*

Second, it appears that a steady deflection of the magnetic needle can be maintained by a *variable* electric current, whose vicissitudes of force are uniform and periodical. For it has been shown that the energy of the currents produced by this machine are so exceedingly variable that it is not constant for any two successive moments during the whole time the machine is in motion.

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* See the appendix at the end of this article.

The fact of the needle's steady deflection by such a variable force, although, I believe, never shown before,* is perhaps, no more than what might have been expected, from a consideration that a variable electric current, whose vicissitudes of force are uniform, periodical, and in *rapid succession,* might possibly produce as steady a pause in a deflected magnetic needle, prone to inert repose, as one whose energy is constant and uniform throughout. The mean of the variable being equivalent to an uniform constant electric force in keeping the needle steadily deflected.

The former galvanometer was removed from the circuit and another introduced. Its coil is similar to that of the former, but its needle is astatic, and suspended by a delicate fibre of raw silk. The compound needle occupied seven seconds in each vibration, upon an average of several trials.

The following table exhibits the deflections due to the several velocities of the revolving coil of the machine.

Coil revolved to the	Deflections of the Magnetic Needle due to		
	Three Revolutions per Second.	Six Revolutions per Second.	Twelve Revolutions per Second.
Right.	60°	70°	82°
Left.	52°	65°	70°
Mean.	56°	67·5°	76°

With the greatest speed which could be given to the coil, the mean deflection was 80°.

When a soft steel needle was placed in a spiral conductor joining the polar cells, no magnetizing effects were produced.

Chemical. A piece of unsized white paper was well saturated with a strong solution of hydriodate of potash, and four thicknesses placed on a slip of platinum in connexion with the negative polar cell. A platinum wire joined the other polar cell and the uppermost ply of the paper. One turn of the wheel, or twelve revolutions of the coil, determined iodine about the salient† platinum point which rested on the moistened paper.

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* Professor Cumming had previously shown that a *pulsatory* current produces a steady deflection.

† When an electric current traverses a liquid, which is connected with the rest of the circuit by metals, the latter may very conve-

A strong solution of hydriodate of potash was placed in a small glass, and two terminal metals of platinum wire introduced. Twenty turns of the wheel produced a copious decomposition; the iodine being liberated at the salient terminal. When the solution was mixed with a little starch the liberation of iodine was much more striking.

A piece of unsized white paper was well soaked in a solution of common salt and archil. Two plies of the paper were placed on the terminal platinum foil, and the circuit completed by a platinum wire, one end of which rested on, and delivered, the current to the upper ply of the paper. Twelve turns of the wheel, or one hundred and forty-four revolutions of the coil, at the rate of twelve revolutions per second, produced a red spot under the salient platinum point. Two hundred and forty revolutions of the coil produced a fine red speck.

Twelve hundred revolutions of the coil produced no such effect when the velocity was reduced to three revolutions per second. This latter result is exceedingly important in the theory of electro-chemistry, and will be noticed more particularly in a paper on that subject, now in preparation.

Without altering the last arrangement, a drop of muriatic acid was permitted to redden the paper for some distance about the salient platinum point, and the machine again put into motion. With one hundred revolutions of the coil a white spot appeared under the salient metal. Two hundred revolutions produced a considerable bleaching effect.

A solution of the muriate of tin was placed in the circuit, being connected with the polar cells by copper wires. The re-entering terminal copper became coated with tin in about six hundred revolutions of the coil; with a velocity of twelve revolutions per second. On reversing the direction of the electric current, and consequently producing a corresponding change in the electric functions of the terminal metals, the tin quitted the wire to which it had been attached by the agency of the former current, and the other terminal (now the re-entering) metal became coated with tin from the solution.

niently be called the *terminal metals*. That which is connected with the positive pole of the exciting apparatus, and *from* which the electric matter springs into the liquid, may very conveniently be called the "salient terminal metal," or occasionally the "salient terminal" only: the word *metal* being understood: and that which is connected with the negative pole, and through which the electric matter re-enters the exciting apparatus, the "re-entering terminal," or "re-entering metal."

In this experiment, it is obvious that from the first attachment of the tin to the re-entering terminal wire, a voltaic combination was formed in the solution, the two terminal metals being now tin and copper; and the current generated by this combination would be urged in an opposite direction to that of the current from the machine. Notwithstanding however, this opposing force, the machine current prevailed in carrying on the decomposition, although the re-entering wire was tinned more than half an inch of its length.

A solution of sulphate of copper was placed in the circuit, the terminal metals being platinum wires. No decomposition could be produced with any speed that could be given to the coil.*

Whilst contemplating on the result of the last experiment, it occurred to my mind that decomposition might probably be effected by the combined energies of *two* currents, neither of which *alone* were capable of accomplishing it. And in order to ascertain how far the view which I had thus taken, was correct, a feeble voltaic current was selected to combine with that excited by the machine. The voltaic combination consisted of a platinum and a copper wire, which were twisted together, and their other extremities immersed in the solution of sulphate of copper, and permitted to remain unmolested for half an hour: at the end of which time not a trace of copper could be discerned on the platinum wire, which in this case was the re-entering metal in the solution.

The copper and platinum wires were now untwisted from each other and connected with the polar cells of the machine, their other extremities communicating with the liquid sulphate of copper. The voltaic combination was now as complete as before, but the circuit was lengthened by the two hundred feet of wire in the coil. The wheel was turned in a proper direction to drive the current *from* the copper wire into the solution, and consequently the platinum was thus made the re-entering metal for both currents; viz. that from the machine and that from the voltaic combination, both of which had to traverse the two hundred feet of wire in the coil, the semi-wheels, mercury in the cells, the terminal wires, and the sulphate in solution. Decomposition was rapidly produced when the coil moved at the rate of twelve revolutions per second. With about one hundred revolutions of the coil, half an inch of the platinum wire became completely cased with copper. When the wheel was turned in the opposite direction the copper left the platinum wire.

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* See appendix at the end of this article.

The first of these results is interesting, as it developes a novel mode of accomplishing electro-chemical decomposition by those currents, which, of themselves, are insufficient to make the slightest change in the compound.* The second result is also very curious, though perhaps, of the same character as the former; for the sulphuric acid alone would exert some slight chemical action on the copper coating of the platinum wire, whilst the current from the machine would assist in its expulsion.

A pleasing and highly curious variation of the last described experiment is made in the following manner. When a piece of platinum wire has been coated with copper by the former process, it will answer the purpose of the copper wire in the solution, and will, in combination with a clean platinum wire, form the auxiliary voltaic combination. So that by removing the copper wire which had previously been used, and putting a clean platinum wire in its place, the latter may be coated with copper by a few turns of the machine in the proper direction. At the same time that wire which had before been covered with copper will now appear quite clean.

This being done, there is again a voltaic combination, but in the reverse order to the former. Now change the direction of the wheel's motion, and the machine current will conspire with that of the newly formed voltaic pair. The copper again changes places, and that wire which was first coated will now be coated again, and the other as clean as at first. The voltaic combination is now again reversed. Again reverse the direction of the wheel, and another transfer of the copper takes place. These transfers may be made several successive times, but the copper coating becomes less perfect every succeeding transfer, and eventually ceases to appear: showing that the dissolution of the copper, by this means, is decidedly superior to its restoration; a consequence, no doubt, of the machine current conspiring with the action of the acid in accomplishing the solution, on the one hand; whilst on the other, the voltaic current, even at first feeble, becomes rapidly more and more so, by the copper abandoning the platinum wire to which it had been attached: and eventually counteracted altogether by copper accumulating on the other wire.

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* I am well aware that compound voltaic batteries, and also electric batteries of jars, may easily be considered as producing compound currents; but the currents in each of these cases are from one and the same source, and not from different sources of excitation, as in the experiments here described.

It is obvious also from these results, that the action of the acid on the copper is greater than that of the voltaic pair on it. Otherwise there would be a contemporaneous appearance and disappearance of copper on the two wires, until both became coated to the same extent; at which time the voltaic current would cease to exist, and all chemical action terminate. But this is never the case, for the copper invariably disappears from the salient terminal.

Similar phenomena are observed in other metallic solutions. The tin coating of a copper wire, for instance, which has required six hundred revolutions of the coil for its formation, will entirely disappear by one hundred revolutions in the opposite direction; whilst the coating of tin on the other wire is very slight indeed.

A portion of muriatic acid was placed in the circuit, the terminal metals being platina wires. Gas was liberated at the re-entering metal, but in so small a quantity that it required very close attention to perceive it.

When one of the terminal metals was copper wire, and consequently a voltaic combination formed by it and the platinum, gas was liberated at the re-entering platinum by this combination alone, to about the same extent as by the current from the machine. But when the two currents operated at the same time, and conspired with each other, the decomposition was carried on with great promptitude, and hydrogen liberated in abundance at the platinum terminal.

When the machine was turned the opposite way, and the two currents opposed to each other, no decomposition took place. These results are beautiful specimens of electro-chemical action by conspiring electric forces, one of which (the voltaic) alone, is too feeble to maintain a permanent deflection of 3° of the needle of the most delicate galvanometer. With the latter galvanometer already described, the permanent deflection with the voltaic pair alone, did not amount to one degree.

Gold and platina with muriatic acid, form a still feebler voltaic combination than the latter metal with copper, but even with this trifling auxiliary force the machine produced a copious flow of gas from the platinum wire. The gold part of this combination was simply a slip of the leaf gold, which was permitted partly to float on the surface of the acid, and partly to hang by the side of the glass vessel containing it. The slip was touched by a point of a platinum wire, the other extremity of which was connected with the salient polar cell of the machine. The re-entering terminal metal was platinum wire. The voltaic combination was too feeble to produce, perceptibly, either decomposition of the muriatic acid, or permanent deflection of the galvanometer needle already described.

Luminous Phenomena. The spark exhibited by any electrical apparatus can appear under no other circumstance than whilst there is an opening in the metallic part of the circuit; and is necessarily exhibited to the greatest advantage when the machine is undergoing the greatest degree of excitation. Now, with regard to the magnetic electrical machine already described, the opening, if any there be, in the circuit, can occur only between the corners of the semi-wheels and the mercury in the polar cells, about the time of their immersion, or emersion; or whilst they are relieving each other in the circuit. But this takes place when the plane of the coil is coincident with the *neutral plane*, or when the excitation is zero. Hence it is obvious that under these circumstances no spark ought to be seen: a fact which is sanctioned by experiment.

In order therefore to exhibit the spark by this machine, a metallic point, at nearly right angles to the plane of the coil, is soldered to one end of the coil of wire, and caused to leave the surface of the mercury when the coil is suffering the greatest degree of excitation, or when its plane is passing through the plane of the magnet. The other end of the coil wire terminates with its two semi-wheels which run in the same mass of mercury as the point moves through; and the spark is seen when the point leaves the surface of the mercury, or at the moment of interruption in the circuit.

Two such points on opposite sides of the axial spindle give two sparks each revolution of the coil: and when the velocity of the latter is considerable, a rapid succession of brilliant sparks is produced.

Effect on Animals. With the last described arrangement, and a simple contrivance for transferring the current from the point, when leaving the mercury, through a person in connexion with the extremities of the coil, a series of shocks are produced which affect the arms to above the elbows. The greater the velocity of the coil, the more frequent, and more powerful the shocks are produced.

By means of a pair of medical directors, a well known electrical apparatus, the shocks may be administered to any part of the body, where access, either directly by the balls, or indirectly by intervening moisture, can be had to the skin.

Recently killed rabbits, and other animals, are convulsed by the current of this machine, in as decided a manner as by the current of a voltaic battery.

Variation in the structure of the Coil.

When the preceding described results had been fully ascertained, by frequent repetitions, the coil of wire was taken off the reel. It was then doubled by bending it in the middle, without breaking, and again coiled on the reel, by laying its strands side by side all the way from the bend in the middle, (which now became one end of the coil,) to its two extremities, which formed the other end of the coil. The length of the circuit through this coil was consequently one hundred feet, or just half of the length of the circuit in the former coil; having two channels in place of one. The extremities of this coil were properly connected with the discharging system of semi-wheels.

The preceding experiments were repeated with this double coil, and the general results were as follow: the velocities of the coils being the same in both sets of experiments.

The magnetic deflections nearly the same as before, being a few degrees greater with the single needle, but less with the astatic.

The chemical decomposition less
The shocks very much feebler
The spark brighter
} Than with the single coil.

Second variation in the structure of the Coil.

The wire was again taken off the reel, and after bending it in the middle was replaced: being now a four-stranded coil. The circuit through this coil was fifty feet, or half of the former: but consisting of four channels for the electric currents to flow through. The ends of the wire being properly connected with the discharging system, the following experiments were made.

Solution of sulphate of copper was placed in the circuit, and platina terminal wires. No decomposition could be produced.

With one platina terminal, and the other copper, the decomposition was exceedingly feeble; and no complete coating of copper could be given to the platinum terminal. Two hundred revolutions of the wheel, or twenty four hundred of the coil, at the rate of twelve per second, gave very little effect: merely a yellowish brown tinge was given to the wire.

With muriate of tin in the circuit and copper terminal wires, twelve hundred revolutions of the coil did not produce as much coating of tin on the receiving terminal, as six hundred by the double coil. The reverse current did not displace the tin coating in 2400 revolutions. It perceptibly diminished it, but no more. The other became a little coated.

With muriatic acid and platina terminal wires, no stream of gas could be produced; but the re-entering terminal, after 600 revolutions of the coil, got covered with minute bubbles, which clung to the wire, and could only be perceived by the assistance of a magnifier.

When the salient terminal was copper, and the re-entering one platinum, the gas was liberated from the latter, but in very small quantities when compared with those produced by the preceding forms of the coil.

The spark was not perceptibly different from that produced by the double coil.

The magnetic deflections rather greater than by the double coil.

No shock could be felt from this coil with any velocity which could be given to it.

Third variation in the structure of the coil.

The wire being taken off the reel, was again doubled, and replaced in a coil of eight strands. The length of the circuit being now reduced to twenty-five feet: the following are the general results obtained from it.

The magnetic effects not perceptibly different.

The spark also, about the same as by the last coil.

No shock could be produced.

With sulphate of copper in the circuit no decomposition could be effected, even with the aid of one copper terminal. Two thousand four hundred revolutions of the coil were tried with the greatest speed which could be given to it, but without effect.

With muriate of tin and copper terminal wires, no decomposition. 2400 revolutions were tried with the greatest speed which could be given to the coil.

With muriatic acid and platinum terminals, not the slightest trace of gas could be produced. When one of the terminals was copper wire, no effect over that of the voltaic current, by the copper and platinum combination, could be observed.

REMARKS.

One of the principal points to be gained in bringing the magnetic electrical machine into general use as an instrument of experimental research, was that of combining the reciprocating currents, excited in the coil, and causing their energies to conspire in other parts of the circuit, where various apparatus for experiment could be conveniently introduced. And that this object has been accomplished by the efficacy of the discharging arrangement which has been described, appears amply attested both by the deflections of the magnetic needle, and by the exactness of the chemical decompositions; and fortunately, similar arrangements, either with, or without the employment of mercury, can be applied to any other form of magnetic electrical machines.

In the series of experiments already described, one and the same wire was used in every form given to the coil. The *lengths* of the electric channels decreased in a geometric progression, and the *number* of those channels increased in a similar ratio; so that the product arising from the number into the length, gives, in every case, one and the same quantity. But the results of these experiments show that the same length of wire, in these different forms of the coil, moving with a constant velocity, and in the same exciting medium, or constant magnetic force, produces very different electric effects.

The electro-magnetic forces appear to be somewhat exalted by multiplying the number of channels, and shortening their lengths; whilst the chemical forces, on the other hand, were as evidently diminished by similar arrangements of the coil; and on some compounds, were entirely annihilated by the last form given to it, or when the *number* of channels was greatest and their lengths shortest.

The effects on the animal system also, lessened with every diminution in the length of the excited circuit; and in this respect, corresponded with the chemical effects. The spark was not much different, whatever form was given to the coil.

By contemplating electric shocks as the effects of a mechanical action on the animal frame, we obviously arrive at this conclusion; that when the quantity of the electric matter is constant, the violence of the shocks will depend upon the velocity with which that matter is transmitted. This reasoning on a former occasion, led me to experiments, the results of which, were in strict accsrdance with the conclusion here drawn.* For by lessening the velocity sufficiently, an infant might be placed in the circuit, (without experiencing much inconvenience) of a discharge of a given quantity of fluid, which, if transmitted with great velocity, would knock down the stoutest man.

Now the velocity of the electric matter through any given conductor, from one side to the other of a jar, depends upon the elasticity given to that matter by the exciting process, whatever it may be: and the elasticity depends upon the density or intensity as it is frequently called. But with the magnetic electrical machine the shocks have obviously some dependence upon the *length* of the excited part of the circuit: hence we may justly conclude that the *velocity* given to the electric matter, depends upon the number and magnitude of exciting impressions given to the coil in a given time.

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* Phil. Mag. vol. 67.

The chemical effects obviously depend upon the same conditions.

If now we consider that the excitation is accomplished by a series of impressions by the magnetic lines of force through which the coil travels: then, if those impressions were of uniform intensity, the velocity of the electric fluid which they put into motion, would be proportional to their number in any given time. And this would be the case whether those impressions were contemplated individually or in uniform groups.

It so happens, however, by this mode of excitation, that the exciting impressions vary in intensity in every part of the circle of revolution of the coil. But, notwithstanding, this circumstance will not affect our reasoning on the total effect produced with different lengths of the excited channel, provided we take, as an unit of excitation, the *aggregate intensity* of the impressing magnetic force exercised during one entire revolution of the coil.

Moreover, it will be convenient for the purpose of arriving at some definite conclusion in a simple manner, to consider the coil to be revolving with an uniform velocity; and that every convolution of the wire suffers the same degree of excitation, which, perhaps, is not very far from the truth.

Under these considerations then, it follows, that, as in every form of the coil there was precisely the same number of convolutions of the wire, there would also, in every case, be the same quantity of fluid put into motion, when the speed of the coil was constant.

This being the case, let c be the number of convolutions in the first form or single coil; let p represent the impressing magnetic force on each individual convolution during one entire revolution of the coil; and V the velocity of the excited fluid when the speed of the coil is uniform. Then, as the velocity depends upon the impressing or impelling force

$$c\,p = V \quad \text{the velocity in the single coil}$$

$$\frac{c\,p}{2} = \frac{V}{2} \quad \ldots\ldots\ldots\ldots \quad \text{double coil}$$

$$\frac{c\ p}{4} = \frac{\mathrm{V}}{4} \quad \text{.......... four stranded coil}$$

$$\frac{c\ p}{8} = \frac{\mathrm{V}}{8} \quad \text{..........eight stranded coil}$$

and $$\frac{c\ p}{n} = \frac{\mathrm{V}}{n} \text{ when the wire is in } n \text{ strands or distinct coils.}$$

Hence, by this mode of reasoning, it appears that the velocity in each individual strand of the wire, which in fact was a distinct coil, ought to be as the number of its convolutions; a conclusion which appears somewhat agreeable to the results of the experiments.

APPENDIX.

A few days after this paper was read before the Royal Society, it was placed in the hands of Mr. Christie, as one of the Council, for perusal; who was pleased to give it particular attention. Mr. Christie suggested to me the probability of magnetic deflections, by this machine, being traceable to a determinate law, relatively to the velocity of the revolving coil; viz: the tangent of deviation varies as the square root of the velocity: a law which appears very distinguishable in the first table of experiments at page 9: where, (as Mr. Christie has remarked) "the fractions $\frac{\text{tan. deviation}}{\sqrt{\text{(velocity)}}}$ are respectively 4663, 4505, 4502. The agreement in the last two is very striking:" a circumstance neither sought for in the experiments, nor thought of whilst describing them.

The results given in the second table, page 10, are not, however, in accordance with the above law; nor am I certain that much accuracy can be obtained with machines of this kind, whilst mercurial cells are attached to them; for the centrifugal force of the semi-wheels causes them to lift up a portion of the mercury above the general surface, and thus prevents their interchange taking place at the proper interval, or whilst the coil is passing through the neutral plane.

The inconvenience of mercury in magnetic electrical machines has been experienced by every one employing them; and in experiments of nice philosophical research, it becomes absolutely necessary to dispense with it; and in *every* process, it is desirable to employ the whole of the fluid excited. Both these objects have been accomplished by the introduction of discharging pieces, or springs, lubricated by sweet oil whilst pressing upon the metallic arcs attached to the revolving spindle.* The first account of this contrivance was given in a paper of mine which appeared in the London and Edinburgh Phil. Magazine for 1835; but I had used it for more than a year previously, as will be seen by the following extract.

"By referring to the Number for November last, it will be seen that I had, some time previously, succeeded in producing electro-dynamic phenomena, of various classes, by giving to magnetically excited electric currents one uniform direction through the terminal conducting wires, by means of a certain contrivance which may very properly be called the *Unio-directive Discharger;* because it has the power of uniting, and discharging, in any *one* direction, those currents which, in consequence of the mode of excitation, are originally urged, alternately, in directions opposite to each other.

"Without some arrangement for this purpose, every magnetic electrical machine in which coils of wire form the original source, would have remained comparatively useless; and those phenomena, the most interesting in electro-dynamics, could never have been produced by the opposing currents, however powerful, rushing from these copious sources of electric action.

"To exhibit the spark, heat wires, or to produce the shock, it matters not in which direction the current flows, nor whether it reciprocates, or proceeds in one uniform direction.

"Electro-magnetic phenomena may also be exhibited by reciprocating currents, or even by opposing currents, provided the force in one direction sufficiently predominates over that in the other. But in the production of chemical decomposition, with exact polar arrangement of the liberated constituents, it requires that the electric currents be not of a reciprocating character.

"It is, moreover, a particular object of the experimenter, in every electro-dynamic process, to avail himself of as much as possible of the excited electric force; and also to prevent, as far as he can, the existence of any counteraction whatever.

"Now, in well-constructed magnetic electrical machines, the reciprocating currents are nearly of equal force, and the predominancy, if any there be, can never be calculated on as a disposable force, as regards either *degree* or *direction.*

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* When oil is not employed, the semi-wheels and spring cut one another, and become rough in a very short time, producing a disagreeable jarring noice: but with oil, no noice is heard. The oil is also useful on other accounts. It extends the breadth of the line of contact on both sides of the spring, and thus increases the conducting surface, affording a more copious flow of the electric fluid; as experience amply proves.

"Besides, it would be exceedingly unscientific, in cases where power is wanting, to employ a *part* only, when the whole is available; or, as in the present instance,* to employ the *difference* only, instead of the *sum* of the reciprocating electric forces.

"Hence the obvious advantage of the *unio-directive discharger,* which places the whole of the excited force at the disposal of the experimenter, and gives to the magnetic electrical machine a degree of importance which it could never have possessed without it.

"The experimenter also, by this means, may safely confide in his predictions, and vary his exhibitions in any way he pleases, as far as the energy of the currents will permit. He is thus relieved from all those corroding apprehensions and mortifying disappointments, which must ever molest his efforts, agonize his feelings, and chill the ardour of his enquiries, whilst operating with an apparatus over the powers of which he has not the slightest control.

"*The following particulars may possibly be interesting to those engaged in magnetic electricity.*

"I produce electric shocks, sparks, steady deflections of the needle, electro-magnetic rotations, &c., and chemical decomposition with exact polar arrangement of the liberated constituents, by the following forms of magnetic electrical machines.

"1. By revolving coils of copper wire between the poles of either a horse-shoe or a compound bar magnet, so as that the wire may strike, at right angles, the most formidable group of magnetic lines, as shown in my theory of magnetic electricity. (See Lond. and Edinb. Phil. Mag., vol i. p. 31.)†

"With the exception of my revolving discs, described in the Phil. Mag. for April, 1832, (Phil. Mag. and Annals, N. S., vol, xi. p. 270.), this is the oldest of my magnetic electrical machines. But for want of a sufficiently powerful magnet it was a long time before I had much satisfaction from it. I have more recently been better provided, and I find that it acts well; and appears to me to be better calculated for some points of enquiry than any other form I have yet seen.

"2. By revolving coils of wire (having an iron axis or armature) in front of the poles of a horse-shoe magnet. My first revolving armature was simply a straight piece of iron carrying a coil of wire, and revolving in a horizontal plane above the poles of a vertical electro-magnet. The idea of this form occurred whilst Mr. Watkins was describing to me the well-known apparatus of M. Pixii, an account of which had reached him a short time before. With this form I never did anything more than produce a feeble spark.

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* This part alluded to machines with a revolving disc and double point discharger: the only kind then known in London.

† This theory is fully explained in these Annals. Vol. I. pages 198, 251.

"In the autumn of 1833, I first saw, in its present state, the splendid apparatus in the Adelaide Exhibition Rooms, made by Mr. Saxton. This modification of M. Pixii's magnetic electrical machine far exceeding in power my puny arrangement. I have, from that time, employed bent armature and two coils in the manner of Mr. Saxton.

"3. Bent armature and coils similar to No. 2. The magnet vertical, and the revolving axis carrying the armature and coils at right angles to the plane of the magnet.

"4. Similar to No. 3, with the exception of a second piece of armature, with its coils, revolving on the same axis on the opposite side of the magnet. The greatest power is obtained when the pieces of armature are placed at right angles to each other.

"5. By fixed coils on the two branches of a horse-shoe magnet, and a short thick piece of soft iron revolving in front of the poles. This is a very neat form.

"6. By four cylinders of soft iron, with their coils, permanently fixed to the poles of the magnet, one on each side of each pole. The excitation is carried on by a revolving piece, as in No. 5.

"My *unio-directive discharger*, which can be applied to any of the above forms, is by far the most happy contrivance I have yet hit upon in this class of apparatus. It consists principally of four or more semicylindric pieces, properly attached to a revolving spindle.

"The mercury, which has hitherto held so distinguished and important a situation in the discharging part of magnetic electrical machines, but which is a complete nuisance to the operator, I have, in most processes, entirely dismissed, by the introduction of my newest forms of discharger."

Shortly after the experiments described in the preceding paper were made, the machine was furnished with the discharging apparatus last mentioned, and with an addition of one hundred feet of wire to the coil.

Its chemical and phisiological energies are much improved by these means. It decomposes water and other compounds which would not yield to it before, and produces shocks of considerable power.

As this machine is not actuated by any versatile magnetic force from soft iron, it is more suitable for philosophical research than any other form I have yet heard of. It was contrived from the views which I have long taken of magnetic electricity, and operates upon the principles of the theory already laid down in the first volume of these Annals. It is, I believe, distinct from all other machines hitherto described; and, at the lecture table, can hardly be dispensed with in future.

Another series of experiments with an improved machine of this kind will be given in the next number, provided there be room. Also a description of a magnetic electrical machine, with revolving armature, of considerable chemical power, will shortly be described in these Annals.

II. *Researches on the properties of magnetic electrical currents; by* M. AUGUSTE DE LA RIVE, *Professor in the Academy at Geneva, and corresponding Member of the Academy of Sciences at Paris.*[*] *An epitome.*[†]

The author states first that magnetic electrical currents are those which are excited in a metallic wire, by the approaching and withdrawing a magnet, that they are instantaneous, and driven alternately in opposite directions.

In the first §, he takes a general survey of magnetic electrical currents. After having briefly described the apparatus by which he has succeeded in producing an uninterrupted succession of these currents, he studies the influence that the velocity with which they succeed one another, might have over the intensity of their effects. He indicates among other results, that the spiral of a metallic thermometer was heated to 7° when there were only two alternately opposite currents in a second: to 55° when there were nine currents: to 100° when there were 20 currents: to 133° when there were 40 currents: and that he even made a fine platinum wire red hot when the succession of currents were more rapid. The chemical effects were submitted to the same influence, only they had a limit to the most advantageous velocity, beyond which the decomposition was retarded. Thus, for example, to produce the same quantity of gas from the decomposition of water, we must have,

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[*] From the "Comptes rendus hebdomadaires des séances de l'Académie Royale des Sciences, No. 22, May 29, 1837."

[†] Translated by Mr. J. H. Lang.

1050 currents when there are	14 in a second
462	23
442	42
400	47
494	52

Whence it follows that the influence of the velocity with which the currents succeed one another, does not consist only in there being a greater number of currents in a given time, but chiefly that the *individual* intensity of each current experiences a considerable augmentation.

This influence of the velocity is also perceived on the physiological effects, which acquire a very superior energy to what they possess when they are produced by voltaic currents; a phenomenon which may be attributed to the discontinuation and alternately opposite direction of the magnetic electrical currents, and of which it may perhaps be possible to take advantage in the art of war.

The object of § 2 and 3 is the study of the passage of magnetic electrical currents when traversing metallic and liquid conductors. The resistance that these currents experience when the length of the most perfect metallic or liquid conductor is increased is considerable, and much greater than that experienced by electrical currents from other sources. But if the conductor be heterogeneous instead of homogeneous the resistance is much less: the contrary to this takes place with regard to other electric currents. Thus a wire one metre long having one end of iron and the other of copper, does not conduct magnetic electrical currents as well as a wire of the same length and diameter composed of four, and still better of eight ends, alternately iron and copper. Acidulated water placed in a glass vessel, conducts magnetic electrical currents quite as well when it is divided into two or more compartments by platina diaphragms, as if it formed one continued mass, provided only that the liquid conductor be not lengthened by the presence of the diaphragms.

In § 4 the author is occupied with the influence exercised on magnetic electrical currents, by the extent and form of the metallic conductor used to transmit them to the liquid. He remarks that the gas which is abundantly evolved when the metallic conductors are straight plates or simple wires, becomes little or nothing, (all other circumstances remaining the same) when these conductors are plates whose surface presents rather a considerable extent, of from four to eight centimetres square at least.

To find out this phenomenon, he placed in the circuit acid solutions of different degrees of concentration, on the one hand by means of a plate of platinum which he could immerse more or less in the liquid, on the other by means of a platinum wire which he could surround with a glass to collect the gas disengaged at its surface. The spiral of the metallic thermometer was in the circuit, in proportion as the plate was immersed in the liquid, the quantity of the gas liberated at its surface diminished, whilst with the contrary there was a stronger development of gas on the wire and an elevation in the temperature indicated by the spiral; and when the extent of the surface of contact between the plate and the liquid had become such that there was no longer a liberation of gas on the surface of this plate (it was then 450 millimetres square in sulphuric acid increased by nine times its volume of water) he found that he had attained the limit of increase in the intensity of the current transmitted; when he even immersed the plate twice or four times deeper he obtained neither more heat in the spiral nor more gas in the wire. He replaced the platinum wire by a second plate of the same metal, and giving it a surface of contact of 450 millimetres square he had no more liberation of gas either from the one or the other, and the metallic spiral indicated a maximum temperature of 46°.

In another experiment in which a still more conducting liquid and platina plates of a greater surface were employed, he was enabled to raise the temperature of the spiral to 93° without the current (capable of producing such an effect,) showing the least decomposition of the acidulated water that it traversed.

There seems to result from the above, that only the chemical effects of the current, are, like the calorific effects, manifested in proportion as this current is impeded in its passage, and at the points in which it experiences this impediment; and as with voltaic piles, the quantity of electricity produced is as considerable as ever, or very rarely less, it cannot entirely flow through the conductors which connect their poles; wherefore he conceived, when these conductors are liquid, however great may be the extent given to the metallic surfaces immersed in the liquids, the current always experiences an impediment and causes a chemical decomposition. With magnetic electrical currents, whose original intensity is much less, we may expect on the contrary easily to attain the limit beyond which these currents experience no more impediment in passing over metallic surfaces in liquids; a circumstance which explains why the interposition of one or more diaphragms does not diminish the facility of their transmission.

The simultaneous employment of liquid and metallic conductors, which forms the subject of the § 5, presents some interesting phenomena, particularly with regard to the theory of electricity. The current transmitted by means of two large platina plates, through acidulated water placed in the circuit, raised the metallic spiral, which it also traversed, to 82°. Without removing the liquid, the two platina plates were connected by a metallic wire, so that the current has two ways instead of one of arriving at the spiral, that of the liquid conductor as before, and that of the metallic wire. If this wire be of silver ½ millimetre in diameter and 45 centimetres long, this double way does not change the effect of the current which continues to increase the temperature of the metallic spiral to 82°. If a greater length be given to the wire the temperature of the spiral decreases and attains a minimum of 67° when the wire is 4 metres long. Then lengthening the wire still more, the spiral is heated afresh and returns to 82° when the wire is 12 metres long.

The preceding results and others of the same kind which have been obtained by varying the nature and dimensions of the conductors employed, permit us to assert the two following principles; 1st that one current driven in the same direction as another, can either increase or diminish the intensity of the second, according to the relations which exist between the ways they have each traversed, when setting out from the same source they arrive at the same point. 2nd. to produce the same effects on a current which always traverses the same way, that traversed by the other ought to be as much longer as it is a better conductor. It is easily perceived that the phenomena we have just described, are the true phenomena of interferences which lead us necessarily to admit that the electric current is propagated by means of very long undulations, and whose length is as much more considerable as the medium in which the propagation takes place in the better conductor.

The ordinary voltaic currents cannot give rise to the same effects, because the sources whence they are derived has such an intensity that the addition of a second conductor instead of causing a division of the same quantity of electricity between this conductor and the first, causes a more considerable quantity of this agent to flow, which renders the results less comparable.

§ 6 is devoted to the explanation of particular phenomena presented by the surface of the metals which transmit the magnetic electrical currents through a liquid conductor.

When acidulated water is decomposed by these currents, making them penetrate the liquid through the medium of two platina wires, we perceive, the liberation of gas, which is at first considerable, diminish, till at length it entirely ceases. In the mean time the currents lose nothing of their intensity; on the contrary they become stronger, of which the temperature indicated by the metallic spiral placed in the circuit is a proof. If we examine the platina wires when the gas ceases to be liberated at their surfaces, we shall find them covered with a thick black stratum, which is nothing but metallic platinum minutely divided, as may be easily certified by several means, and particularly by the faculty which a wire covered with this stratum possesses of determining the combination of gas on being introduced to an explosive mixture. *Gold* and *palladium*, under the same circumstances present the same phenomena as platinum; they are also covered but more rapidly with a minutely divided stratum, possessing the same properties; it is the same also with metals acted on by conducting solutions, such as *silver*, *copper*, and *lead*.

We have carefully collected and measured the gases liberated when a magnetic electrical current is transmitted through divers liquid solutions, either by means of platina or gold wires. The analysis of these gases has proved that they were always *oxygen* and *hydrogen* in the proportions which form water, a fresh proof that the divided stratum was much of pure metal without any oxide mixture. We have observed, further, that in proportion as the volume of the gases developed diminished, the metallic spiral placed in the circuit, acquired a higher temperature, and only arrived at its maximum when there was no longer any production of gas. The current had then its maximum intensity. The different solutions submitted to experiment, presented curious differences as to the quantity of gas liberated, and the heat produced in the spiral traversed by the current, these two effects being generally inverse to each other with regard to their intensity.

The author terminates his memoir with the examination of two questions, intimately connected with one another.

The object of the first is to know whether the absence of gas, when magnetic electrical currents are transmitted through a liquid conductor, either by means of wires covered with a minutely divided metallic stratum, or by plates of large surface, is owing to there being really no decomposition, or to the oxygen and hydrogen resulting from the decomposition ariving nearly at the same time on the surfaces, and being recomposed by the influence of these surfaces. Some facts mentioned in the memoir would seem to favour the second opinion, which, however, particularly as to the plates, seems to be less probable than the first.

The second question relates to the cause, which, in the above mentioned experiments occasions the surfaces of the metals to be covered with a minutely divided stratum. Is this effect due to the alternate liberation of the oxygen and hydrogen on the suface of the metals, causing them to experience so multiplied a succession of oxidations and disoxidations, that at length there results a disagregation of the metal itself? Would this explanation, which also accounts for the effect of platinum sponge, and generally for the divided metals on the explosive mixtures, admissable for oxidizable metals, and even for gold, be accepted for platinum, which we must then regard as susceptible of combining directly with the oxygen, under the influence of certain circumstances? Or might it not probably be (and this cause even though it should not be the only one, might contribute much to the production of the phenomena), that the very rapid succession of instantaneous and alternately contrary currents, produced at the moment when these currents passed some metals in the liquids, shocks violent enough gradually to effect the disagregation of the particles of the metallic surfaces? This conjecture would seem to be confirmed by those metals which have the most tenacity, platinum, and above all iron, offering the greatest resistance to the disagregation. Besides, these shocks might be rendered visible, particularly with mercury, which being liquid, instead of experiencing a disagregation at its surface, exhibits, when it conducts the magnetic electrical currents through a liquid, extremely rapid vibratory motions of a much more marked and general character than those to which it is subject, when used for the negative pole of a voltaic current. There are also perceived about the wires, particularly those of silver, when they are immersed in a liquid through which they conduct the magnetic electrical currents, a series of vibrations which parting from the immerged surface, are propagated in the liquid. Gold and silver wire only exhibit this phenomenon when they are covered with their divided and very thick stratum; it is also necessary, in order to render it very visible, that these currents do not succeed too rapidly.

The author after having shown that these last facts approach those that he has described above, and relative to the kind of interferences, of which the currents are susceptible, are naturally to strengthen the opinion that the electric current is propagated by undulation, announces that he is going to endeavour by means of as precise instruments as he can procure, to compare numerically more correctly than the results he has already obtained, and to measure the length of the electric undulations.

III. *On chemical reactions produced by the contact of oxidizable metals with distilled water and insoluble compounds. By* M. BECQUERELL.[*]

On the action produced by electricity of a low tension on insoluble substances.[†]

In my former communications I have frequently had occasion to show how, by the assistance of electro-chemical effects, we could promote among bodies, which are present, affinities that could never be produced by the ordinary chemical means, of which the observations I have had the honour to present to the Academy are an additional proof.

Hitherto we have used, for the decomposition of insoluble substances, electric currents proceeding from voltaic apparatus, composed of elements more or less considerable, but we can act equally on many of these substances by employing simultaneously the affinities and action of currents produced by the slow reaction of two bodies on each other. We know, in short, that if the electric powers, by virtue of which the elements of a body are combined, could be turned into a current, it would have the necessary intensity to accomplish the separation of these same elements. But when two bodies are combined together, the electricities liberated, represent those which constitute the electric power. Hence if it were possible to transform them into a current, it would effect the separation of as many elements as entered the combination, but we could only effect this transformation on a very small portion of the two electricities disengaged, since it produces in the liquid many recompositions which much diminish the intensity of the principal current; whence the more the number of these recompositions is decreased, the more the intensity of the current is augmented, and the more therefore it tends to become equal to that of a pile composed of a certain number of elements. This condition is fulfilled by disposing the apparatus so that the electricities disengaged, should traverse the smallest possible space in the liquid.

To give an idea of the general method spoken of, we shall relate a series of experiments which will show at the same time the advantage that may be derived from this new mode of decomposition in the formation of different products, several of which have not yet been obtained by the ordinary process of chemistry.

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[*] Comptes Rendus hebdomadaires des séances de l'Académie Royal des Sciences, No. 22, May 29.

[†] Translated by Mr. J. H. Lang.

First experiment. In a tube of about a centimetre in diameter, closed at one end, was placed some newly precipitated oxide of copper, distilled water, and a plate of zinc. In the space of a week or two, the following reactions were observed; the oxide assumed by degrees a green tint by combining with the carbonic acid of the air through the medium of the water: a portion of the carbonate was decomposed by the zinc, the oxide of copper was reduced, and the plate covered, at the part in contact with the oxide, with small anhydrous crystallized grains of carbonate of zinc, whilst flakes of the same compound were deposited on the upper surface. Bubbles of hydrogen gas were at times liberated owing to the decomposition of the water; substituting carbonate of copper for the oxide, the results were the same. We cannot but perceive in their production the influence of electric forces liberated by the reaction of the water on the zinc; we will quote hereafter other examples of this influence. Iron in contact with water and the protoxide of tin, reduces the latter. Other oxides are equally reduced by iron and water.

Second experiment. In a tube four millimetres in diameter closed at one end, was placed a demigramme of black sulphuret of mercury, on which a saturated solution of marine salt was poured; a plate of copper was then immersed to the bottom and the tube hermetically sealed. Although the sulphuret of mercury is not soluble in marine salt, and the latter does not sensibly attack the copper, excluded from the air, nevertheless from the different feeble chemical reactions which took place on the contact of the copper with the sulphuret of mercury, the water, and the chloride of sodium, the following results were developed: decomposition of sulphur, formation of eight sided crystals of mercury, combined, probably, with a small portion of copper, on the copper plate and side of the tube. The operation which commenced eight years ago has continued without interruption, and it is probable that in time all the sulphur will be decomposed.

If more prompt effects be required, salt water must be substituted for distilled, the end of the copper plate which is put in contact with the sulphur must be amalgamated, and the tube kept open. In a few days the effects of the reaction will be sensible; the plate will, by degrees, become covered with crystals amalgamated with mercury and copper. It appears that under the influence of the air, sulphate of copper and sulphate of mercury which are reduced by the action of a voltaic pair of copper and mercury are formed simultaneously. The crystals being grouped in confusion beside each other, it is difficult to determine their form. Without amalgamating the end of the plate in contact with the sulphur there would be no sensible reaction produced in the space of a month.

Third experiment. Operating in the same manner with sulphuret of copper, distilled water, and a leaden plate, we obtained the following results; slow formation of sulphate of copper, which was dissolved; gradual decomposition of this salt by the lead, and the formation of sulphate of lead which forms eight sided crystals semi-prismatic variety of Haüy.

Fourth experiment. The reaction of oxidizable metals even on the most insoluble salts is such that, the sulphate and phosphate of lead, in contact with an iron plate and distilled water in a glass tube, suffered decomposition; the lead was precipitated on the iron, and produced on one part sulphate of iron, which was found in the water; on the other, sub-sulphate and phosphate of iron which were precipitated.

Fifth Experiment. In a tube of a centimetre in diameter carbonate of hydrated copper, a saturated solution of marine salt, and a plate of iron were placed, and the tube hermetically sealed. By degrees the carbonate, from its original blue, became black; the plate was covered with metallic copper, and at the end of some months the decomposition was complete. The solution of the marine salt became green from the presence of the chloride of iron.

Whence it is beyond a doubt, that in the different reactions which take place by the contact of water, marine salt, carbonate of copper, and iron, the hydrated carbonate was at first decomposed under the voltaic influence in water and in anhydrated carbonate, then the latter was completely decomposed. When the experiment was made in contact with the air the carbonic acid was disengaged and precipitated from the oxide of iron.

Substituting a leaden plate for the iron one, the carbonate of copper was equally decomposed, and a double chloride of lead and sodium was formed, which was crystallized in pretty rohomboids of the carbonate of lead, probably chloro carbonate in acicular crystals; but all these crystals are so mixed with each other, that it is very difficult to separate them. Plain soda mixed renders the liquid slightly alkaline.

Sixth Experiment. Carbonate of silver, distilled water, and a leaden plate, were arranged as before: the carbonate was soon decomposed; a part adhering to the side of the glass formed in different places a continued metallic contact, as brilliant as if the glass had been tinned. The leaden plate was covered at the same time with hydrated carbonate of lead, in small mother-of-pearl plates.

This carbonate, like that of copper could only be decomposed as long as the electric effects produced by the oxidation of the metals, in contact with the water and the air, intervene in this reaction, to effect the separation of its elements.

Substituting a plate of copper for the lead, the carbonate of silver was still more rapidly decomposed; it formed a green carbonate of copper, which by degrees changed into blue. The side of the tube was covered with tolerably clear small crystals of this carbonate. The silver reduced was mixed with small crystals of protoxide of copper.

The electro chemical theory relates also various reactions produced by an apparatus in which are carbonate of copper, lead, and marine salt.

Bodies present and products formed.	**Atomic compositions.**
Bibasique carbonate of copper	1 atom of deutoxide of copper
	1 ——— carbonic acid
	2 ——— copper
Chloride of sodium	1 ——— of sodium
	2 ——— of chlorine
Lead	
Water	
Chloride of Lead . .	1 atom of Lead
	2 ——— of Chlorine
Carbonate of lead .	1 atom of protoxide of lead
	1 ——— carbonic acid
	Probably 2 atoms of water
Double chloride of lead and sodium . . .	1 atom of chloride of sodium
	1 ——— chloride of lead

One atom of sodium being liberated, the liquor becomes alkaline.

We see by this table that all the separate elements, with the exception of one atom of sodium liberated, enter into the new combinations.

Operating with chloride of calcium and iron in a closed tube, the effects are the same, except that it deposits an ochrated matter, compounded probably of a double carbonate of lime and iron.

Substituting salt water for distilled, we have hydrated carbonate of lead in small plates.

The silicates of silver and copper, as well as the aluminates, are equally decomposed when they are submitted to actions analogous to those used in the preceding experiments. The effects produced could not be foreseen *a priori.*

Seventh Experiment. Silicate of copper, distilled water, and a plate of zinc, were disposed of as before in a tube several centimetres in diameter, and closed at one end. The whole of the plate was covered in a short time with small tubercles of a well grounded blue colour, compounded of blue carbonate of copper and anhydrated deutoxide; whilst on the lower part of the plate were deposited copper and crystalline grains of carbonate of zinc; the silica was deposited singly, and at the same time hydrogen gas was liberated in tolerable abundance.

These reactions could not be foreseen, because they resulted from unknown relations existing between the electric effects produced by the oxidation of the zinc and the affinities of the different substances which are present.

In reasoning *a posteriori,* we must admit first, that the silicate of copper is soluble to a certain extent in water by means of the carbonic acid in the air; secondly, that this acid under the influence of the zinc, exercises a repulsive action on the silica and water, whence result carbonate of anhydrated copper, oxide of anhydrated copper, and hydrated silica. Thirdly, that in this case water is decomposed; fourthly, that when the oxidation of the zinc increases, the carbonate of copper is itself decomposed, the oxide is insulated or reduced according to the strength of the current; fifthly, that the oxide of zinc combining equally with the carbonic acid of the air, forms crystalline grains of carbonate of zinc.

Substituting lead and iron for the zinc, the effects are different: the silicate of copper is entirely decomposed, there is an immediate reduction of the oxide of copper without producing blue carbonate. We scarcely observe from one time to another the liberation of gas bubbles.

Eighth Experiment. The silicate of silver and even the aluminate submitted to this mode of experiment with the lead, were soon completely decomposed. The reduction commenced at the part in contact with the lead, and spread over the whole mass, which at the end of some weeks was only a metallic powder, homogeneous in appearance. Washing this powder in a tube full of water, it was not separated from the substance, not having the same density: subjected to heat by a solution of potassa, we obtained silicate or alumine and oxide of lead, the silver remaining metallic.

The aluminates probably admit of the same; we have only submitted to experiment a mixture of aluminate and silicate of copper, with a saturated solution of marine salt, and an iron plate. The two salts were decomposed, the oxide of copper was reduced, and there were deposited on the side of the tube, very small limpid crystals with rectangular faces, which can only belong to a silicate or subsilicate of alumine, such as the disthene of Haüy which has for a primitive form, an oblique quadrangular prism whose longitudinal faces are rectangular.

The phosphates and insoluble arseniates of easily reducible metals were submitted to the same mode of experiment, by employing still distilled water, as a medium between the metal and the compounds. The following are some examples of this kind of decomposition. With the arseniate of silver, or at least the subarseniate, distilled water, and lead, there were deposited on this latter small crystalline plates of a white pearly arseniate of lead, and the arseniate of silver after some weeks was transformed into metallic silver; the side of the tube was covered with dendrites of this metal. We ought to remark that the liquor being acid proves that a portion of arsenic acid was liberated.

Tenth experiment. With the subarseniate of silver, distilled water, and copper, there was equally a decomposition of metallic salt, reduction of the oxide, and formation of acicular crystals of arseniate of copper of a soft green.

Eleventh experiment. We have also submitted to experiment the chromate of silver in the hope of forming chromate of lead, like that of Birisof, as we have already obtained by other processes; we used for this purpose chromate of silver, distilled water, and lead. The metallic salt was not slow in being decomposed. There were deposited on the lower side of the glass small crystalline plates of silver. The liberated chromic acid was combined with the oxide of lead, produced by the reduction of the oxide of silver, and there resulted at the first yellow chromate of lead, similar to that which chemists obtain by double decompositions; but afterwards this chromate became red orange like that in nature, and it was crystallized in needles. This change is analagous to that which took place in the sixth experiment where the green carbonate of copper by losing its water became blue; chromate of copper, water, and lead, have given the yellow chromate in needles, proto-chloride of mercury with water, and the copper of the crystals of double chloride of copper and mercury, and metallic mercury.

These experiments varied in a thousand ways, gave new and unexpected results, which cannot fail to interest the chemist and geologist. They prove also that insoluble salts with a metallic base, can be decomposed by simple voltaic apparatus so as to produce combination, some of which cannot be obtained directly by means of chemistry.

Twelfth experiment. We may operate equally on insoluble compounds which do not contain metallic oxides. We will take for example, iodide of sulphur which allows the iodine to be disengaged at the ordinary temperature. If after having ground this compound into very attenuated parts, we put it in a tube with water and a leaden plate, the water is charged by degrees with the iodine and forms crystals of iodide of lead which visibly extend; crystals of iodine are deposited on the lead and the sulphur is imperceptibly laid bare.

The electric effects produced by the reaction of the iodine on the lead assist the decomposition of the iodide of sulphur.

If we substitute for the lead a tin plate, in a tube of small diameter, the decomposition of the iodide of sulphur seems to proceed more rapidly. In the space of 24 hours there is deposited on the plate needles of periodide of tin of a red orange colour, which become yellow without changing their form on being subjected to boiling water.

The iodide of sulphur, in contact with the copper and water, is equally decomposed tolerably quick, there is a deposit of sulphur and formation of iodide of copper.

In another memoir we will examine what passes in the preceding preparations on substituting an organic substance void of oxygen for the oxidable metal. This fact in our opinion may serve to throw considerable light on the natural operations which take place in organised or unorganised bodies.

On the influence of surfaces on electro-chemical action.

It has long been known that if a saturated solution of salt be left to itself, the crystals will be deposited on the sides of the vessel which contains them in preference to other bodies which may be there and particularly the edges. The force which acts in this case seems to be of the same nature as that which causes capillary attraction, and determines in spongy platinum the combination of the hydrogen and oxygen at the ordinary temperature, a property belonging not to unoxidisable metals only, but to all bodies, such as coals, pumice stone, porcelaine, glass, &c., whose temperature is sufficiently raised. The state of the surfaces has so much influence over the results, that we find a very notable difference in the quantities of water formed in the same time from fragments of glass according as they may be angular or rounded. In general, the effects are as much more marked as the bodies have cleaner surfaces, it is thus that the platinum plates used in electro-chemical decompositions determine the combination of the two gases, their surfaces having been cleaned by the action of the elements transported by the currents. It is, by this reason also, that we give the greatest power to platinum by first subjecting it when hot to caustic potassa, then to sulphuric acid, and washing it in distilled water.

Platinum as well as unoxidisable metals and other substances, possesses the property of condensing at their surfaces or in their pores when they are spongy, the oxygen and other gases, a property which does not belong to bodies that combine with the gases. The force in question exercises also an influence on electro-chemical phenomena.

We have already mentioned some examples for the support of this assertion; we are about to relate some others which will strengthen it. We will commence by giving one of the experiments in which we have studied all the circumstances of the phenomena. We take a glass tube closed at one end, 8 or 10 centimetres long and 2 or 3 millimetres in diameter, and place therein calcined oxide of cobalt reduced to a very fine powder, a silver wire, and a solution of hydro-chlorate of chrome; we then close the upper opening. Fifteen days after we begin to perceive metallic dendrites on the side of the tube proceeding from the reduction of the oxide of cobalt which is really affected only on the surface of the glass. This effect equally taking place without the presence of silver, we must conclude that the action exercised by the side is the cause of the reduction; whence contrary electricities disengaged by the slow reaction of the oxide of cobalt on hydrochlorate of chrome, recombine by following the stratum of the liquid which adheres to the side of the tube; therefore this side, or at least the stratum of liquid adhering thereto, must be taken for the negative pole of a voltaic pair whose positive pole is the hydro-chloric acid. It is thus that we view the effects produced. When a body is immersed in a liquid, there is an action exercised by the one over the other, that is to say an action of the solid on the liquid; we have admitted, and this supposition is justified by the agreement of the results of calculation with those of experiment, that this action by reason of which the liquid adheres to the solid is exercised only at an infinitely small distance from the side. That settled, the particles of the excessively small liquid film submitted to this action, must not be in the same state as those placed at a certain distance. Whence, we may say with certainty, that the electric properties of this film must not be precisely the same as those in the portion of the liquid not submitted to the capillary force. But when a body reacts on a solution, both in contact with another solution incapable of exercising on it a chemical action, this serves to constitute a voltaic pair; this is precisely the case.

By the reaction of hydro-chlorate of chrome on oxide of cobalt, there is probably formed a chloride or subchloride of cobalt. The hydro-chlorate takes positive electricity and the oxide of cobalt negative: these two electricities recombine through the medium of the film which adheres to the side, so that the latter constitutes the two poles of a small voltaic pair; the chloride of the one part is decomposed and the other is formed into hydro-chlorate of chrome.

Some examples will show in a remarkable manner the influence of surfaces on electro-chemical phenomena.

In the first experiment quoted, we put in the bottom of a tube, black sulphuret of mercury, a saturated solution of marine salt, and a plate of copper; we then closed the tube; at the end of some months we began to perceive on the side of the tube particles of mercury, which, so increased with the time, that in six years they were about two millimetres in extent. They then formed the rudiments of regular octa-hedrons; on the plate above the sulphur, were formed small octahedrons of mercury, probably combined with a little copper. Here the influence of surfaces is manifest, since the decomposition of the sulphur began precisely at the parts in contact with the glass. Thus the capillary action had sufficient force to retain the mercury and make it lose its liquid state.

In a tube in which we had put carbonate of copper, chloride of calcium, and an iron plate, the upper part of this plate was covered with metallic copper, the carbonate became black, that is to say anhydrated, and the part adhering to the glass was reduced in the form of dendrites.

We might quote many other examples analagous to the preceding.

All the experiments related in this memoir are of much interest in the study of slow actions, and show afresh the advantages that may be derived from the electric effects produced by chemical actions, to give a new energy to these actions and even to create them anew in bodies which are present.

IV. *On the two electricities, and Professor Wheatstone's determination of the velocity of electric light, by* W. Ettric, Esq. *Read before the British Association, for the advancement of Science, at the Liverpool meeting, Sept.* 11, 1837.*

In a paper which I read before this Association, last year, it was shown that the electric fluid in passing through a piece of card, invariably made two or more holes, or one of an elongated figure, thereby rendering the supposition probable, that two or more electric fluids were transmitted. Since that paper was written I have observed a remarkable fact in the spontaneous discharges of some electrical jars, which tends to confirm what was then stated. I believe it is known to electricians that the spontaneous discharge of an electrical jar sometimes leaves upon the surface of the glass an indelible mark, but I am not aware that it has been noticed by any electrician that such lines exhibit any appearances indicating the passage of two electricities, which I am persuaded is really the case. Upon examining the marks left upon these jars, (speaking of the jars left upon the table) it will be seen that what appears at a distance as a single zig-zag line, is really two, going parallel to each other, whatever be their distance; except in two instances as in the case of jars Nos. 2 and 3, where in the first case the electric discharges approach each other, an effect evidently caused by some local attraction, such as an elevation of the tin foil, or a conducting substance, as damp air, being interposed. In the jar No. 3 an evident crossing or passage appears, of which I will speak, further on. If Nos. 1 and 6 be examined it will be seen that, however near the two electric discharges are to each other, there is always a perceptible distance between them, easily distinguished by the eye, as shown particularly by jar No. 6. It is also very remarkable that however near, or however distant these lines may be from each other, it is distinctly seen that they are nearly parallel, if not exactly so, if we except the entering and emerging points, where they are drawn towards each other. It is extraordinary how fully this is verified in all the four jars shown before the section, for by examining them attentively it will be seen, that whenever one side acquires a bend, the other does so likewise; and that however sharp the angle or bend may be in one, the angle or bend in the other is equally sharp; and in a single discharge there may be twenty or thirty, or even fifty such bends, half of them being in the inside of the jar, and the other half on the outside. In the case of No, 3, spoken of before, it may be seen in the most distinct manner, that the two lines or discharges actually cross each other at a point considerably distant from the coating of the jar, shown in fig. 9, Plate II. I contend that we here behold the passage of *two* distinct electricities, and not of one only, as might be argued by any person before seeing the crossing lines of No. 3, which certainly was my own opinion till I had observed those lines. But from that time I freely gave up what could not be maintained by any just argument. The first idea that occurred to me was what I presume would have occurred to any one else, namely, that it was caused by the great dispersion of the electric fluid created by its own repulsive power for the similar particles of electricity or electrified air; but that the glass was not discoloured by it except at the edges where it was in actual contact with the air of the atmosphere, by which alone the discolouration was produced. But it is perfectly obvious that in this supposition two points are maintained, which cannot be reconciled to each other, namely, that there must be the internal line of electrified particles of air so as to obtain the propulsion, and that if such be the case it obliges us to suppose that the whole breadth of the line should have been discoloured, which on strict examination will not be found to be the case. The outer edges of each line on two of the jars certainly do give us the appearance of an outward propulsion, for the lines on those sides are not smooth and even, but rough like the edges of a feather; but as this is not to be discovered in every jar, it seems as if it had been caused by some accidental circumstance as the proximity of a few conducting particles of moist air. It may be very difficult to assign a reason for the difference in the distances of the lines upon the jars, for they are nearly as one to six in the jars 1 and 6, but if I might hazard an opinion on the subject, I would say that the thickness of the glass had something to do with it,* for I find that the jar No. 1, which has the widest lines, has an electrical capacity of 19¾. The jar No. 4, being the second in width, has an electrical capacity of 18. The jar No. 3, whose lines are next in width, has an electrical capacity of 17¼. The jar

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* Communicated by the Author.

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* The most obvious explanation appears to be, that the distance between the lines is some function of the quantity of fluid which produced them. A jar which has much tendency to discharge spontaneously, will do so at very different intensities, according to circumstances; the principal of which, when the jar is not near the conductor, is the hygrometric state of the glass and surrounding medium; and the boundaries of the track marked on the glass, are

No. 2, an electrical capacity of 16½; and the jar No. 6, which has its lines the closest, has an electrical capacity of only 14½. The weights of these jars also nearly correspond inversely with their electrical capacities, for they are respectively 1 lb. 13½ oz. 1 lb. 15¾ oz.—2 lbs.—2 lbs. 0¼ oz. and 2 lbs. 5 oz. with their coatings. There is an appearance in the electric light of accumulated electric discharges that does not appear to have been particularly noticed by electricians, namely, the distinct divisions into which it is divided. It is a very old remark that the light appears of a blueish shade at, or near, the surface of the positive point, and I am not certain whether it has also been noticed that a similar appearance may be remarked at the negative point, the only difference being the actual lengths of such blueish spaces. By attentively observing the spark I found that the middle of it also exhibits a similar appearance generally, though not always. These remarks I made when the spark was taken in an exhausted atmosphere, as well if not better than when taken in the common air. It was particularly noticed two years since, when passing the electric light through the long double legged Torricellian Vacuum, the distance between the two surfaces of the quicksilver being not less than eighteen inches or two feet. The first time I observed it in the Torricellian Vacuum was when the insulation of the mercury cup at the lower part of the tube was imperfect, which prevented the electricity being forced through the intervening space, when it appeared that although the electric fluid could not leap the interruption, it nevertheless passed a part of the way, and that from both ends at the same time, evidently showing the two electricities going, or attempting to go from the two ends at the same moment. Now in the Torricellian Vacuum, and in the discharge through the open air, there appeared to be a considerable difference in the length of the luminous portion within the tube, that at the positive end being twice the length of that at the negative one. The remark was made by me some years since but was then overlooked, having been considered as an ocular deception. This experiment, together with one which will be spoken of further on, I contend show that the different appearances upon the points of wires electrified positively and negatively, consists in the passage of currents in opposite directions at the same moment; for else how are we to account for the electrical

more or less distant from each other, as the discharge takes place from a higher or lower intensity; and consequently from the passage of a greater or smaller quantity of fluid; which facts are in strict accordance with those which Mr. Ettric has observed with different jars. EDIT.

appearance at the negative pole when none could be supposed to have passed; for had it done so, the gradual wearing away of the intensity of the electric light would have appeared from one end only towards the centre, or rather negative end. I have observed an appearance in the *centre* of the spark, given by a jar of about a square foot area, which indicates that the same action is going on in the *central* portion of the spark as at its ends. I may mention that this appearance is best seen in an exhausted atmosphere, and that I never observed the appearance at *one end* and the middle. I do not pretend to show how this alteration of colour and intensity in the spark is produced, that involving the question as to the true nature of electrical appearances, of which nothing at present is known, though we have every reason to presume that we shall shortly be enabled to do so from the splendid experiments now in progress by that truly ingenious gentleman, Professor Wheatstone. A very strong argument has always been held against the supposition of there being two electricities, from the appearances presented by sharp points giving off the electric fluid. To me these appearances seemed to prove the contrary, if they indicated any thing at all; but however this may be, it will be seen that those appearances are very much altered by placing the point in a glass tube having its end drawn out to a fine point. It will in such case be observed that the star-like appearance is wholly done away with, and a brush of light similar to the positive one is produced, though only about half the length and consequently half the breadth.

Though we may point out experiment after experiment which tend to show that this or that theory is correct, nevertheless we will never be able to arrive at any definite conclusion so long as an experiment admits of a double solution. It therefore appeared to me many years since, that the question of one or two electricities would never be set at rest till the actual time of the passage of the electric fluid (if I may be allowed to call it so) could be discovered. After the splendid discovery of the eminent gentleman of whom I have just spoken, it was obvious that at least we would be enabled to set the old question at rest respecting the passage or action of electricity from one or both ends of the circuit, if we could not even do so for the actual velocity. Since that discovery it always appeared to me to be a cause of regret that the Professor did not succeed in discovering the direct method of exhibiting it, and that he was thereby obliged to have recourse to what I would term a secondary one, the method by reflection, which has given the opponents an opportunity of disputing the accuracy of the result, and that not without reason. I see one source of error, namely, the irregularity of the figure of the mirror, which I am not aware has been mentioned as a possible source of error, and one which would not probably be discovered by the reversion of the revolution. I could readily show that what is termed a plane mirror, is in fact only a surface of some regular or irregular figure, not a mathematical plane, which is the thing required. The nearest approach to such plane, which the hand of man can obtain, is a segment of a sphere of a very long focus, and I think that I may state with confidence that with a mirror one inch diameter, the longest focus ever obtained with certainty was by myself with an improved method of working, by which I could procure it of 400 or 500 feet; but this is insufficient for obtaining any perfect result when such minute quantities are to be shown as the duration of the passage of electric light. Moreover with the mirror we do not possess any scale by which the quantity can be accurately measured, it being merely estimated by the eye, which is a very uncertain method, on account of the distance and want of sufficient time to compare the quantity, but more especially by the sudden start which the eye receives from the flash of light. It is also well known that the measures of luminous objects are very different according to the ground or light upon which they are viewed. It is therefore not to be wondered at that the measures by the mirror should have been so different by different observers. I therefore think that the section will see the necessity there was of devising some method of showing, by a direct method, the accuracy of the results as given by the mirror, and I am sure that Professor Wheatstone will not only pardon me for stepping into his path, but rejoice to find that it has been in my power to assist him in proving, in some degree, the accuracy of his results, and hope to be enabled to prove the remainder when the machine shall have been improved by his kind suggestions.

The instrument consists of an axis or spindle moving on two points A and B fixed steel in a strong cast metal frame, figs. 10 and 11. This axis is not formed of one straight circular rod, but has a square frame E F G H, in its centre. In this frame two arms or hollow metallic tubes C M, D L, are soldered, into which glass insulating tubes, C R, D S, are secured by screws and cement. The ends of these glass tubes project half an inch beyond the metal at each end as shown by R and S. A copper wire is passed through each glass tube and filed off level with the ends of them at C and D; the other ends are left rather long, so as to pass from them to the axis of the shaft, the one wire being connected with the upper, and the other with the lower axis, which, by wires P Q, T U, fig. 10 twisted round the glass rods P T, Q U, fixed in the iron rod 8, 10, are made to communicate with the long coils of copper wire. This revolving electrometer is allowed to pass close to three glass tubes Z and X Y, having wires within them reaching to their points, two of these points being placed at one end of the cast iron frame, and the third at the opposite end, directly opposed to one of them as X through the line of the centre. The two points X Y are made adjustable so as to increase or decrease the distance between them. The greatest distance at which they can be placed is about half an inch, and the nearest one-tenth of an inch. The electrometer is revolved with as great rapidity as possible by the wheel W, a strap passing over it, and a larger one driven by a train of multiplying wheels, part of which are shown at fig. 10. The velocity of the electrometer being to the velocity of the moving power, as 2800 to 1. The moving power consisted of the joint efforts of eight men acting upon levers six feet long, by which I was enabled to obtain a velocity of 90 revolutions in a second of time. The arms of the electrometer are 9 inches, making the diameter 18 inches from point to point, and circumference of revolution 56·55 inches.

If the points be then placed at half an inch distance, and the electrometer be revolved at the rate of 80 times in a second, the instrument will show the 4524×2=9048th. part of a second of time for the passage of the point D from the point X to the point Y; and the 45240th part of a second when only one tenth of an inch. The action of the machine is thus:—Let O fig. 11 represent the internal and external coatings of a Leyden jar, and O Z a wire from the internally charged coatings. A discharge will pass through the wire C M (which is insulated by the glass tube) along G to K, 1, 2, where it enters the long coil of wire 2, 6, 3, one-60th of an inch diameter, and, after passing through it, leaves at 3, going on to the point X, where it leaps the interruption, then goes through the wire D, L, F, I, entering the other coil at 4, and going out of it at 5, to the outside of the jar. If we now suppose the electrometer to be put in rapid motion and the electricity be then passed, as here shown, it is obvious that the point D will not be opposite the point X when the electricity arrives at it, provided it takes any sensible time to travel through the wire; but will, if the velocity of the machine is sufficient, be opposite the point Y. Therefore knowing the speed of the electrometer, the distance of the points, and the length of the coil of wire, we can readily obtain the velocity and direction of the electric fluid.

The wire was of that kind called No. 27 of the wire gage, which I find is the 1-60th of an inch diameter, and was well insulated by 141 stout glass rods, 12 inches long. It was 5850 yards long, or nearly three miles and a third, and was divided into two equal portions, of rather more than a mile and a half each, placed on opposite sides of a room thirty feet and five inches long, the lengths of wire being so placed as to lie in flats, no one coming within one inch of its neighbouring length. The method of fixing the wire upon the glass rods is shown in fig. 12, *a b*, *c d*, two deal boards placed upright at the distance of thirty feet five inches. They have eighteen holes each, for fixing the glass rods *e f*, *j k*, *g h*, *l m*. In the drawing, only two glass rods are shown, but it is obvious that the others are fixed below them so as to form a succession of flats for carrying the wire upon. The glass rods are made of bottle glass, 13 inches long, and one inch and three eighths diameter, which is the nearest distance the wires approach each other. The middle of the wires are kept up by light cross glass rods suspended by string; consequently there are 144 glass rods.

When the electrometer revolved at the rate of eighty times in a second, the points being half an inch apart, the electric discharge invariably struck the point or wire directly opposite the entering one, whether the three miles of wire were used as one coil, or as divided into two equal portions, the four ends being connected with the electrometer as shown in fig. 11. Nevertheless, an obvious lateral deflection of the spark was perceived when divided, though it was not noticed when the wire was used as one length, and this was first noticed by a gentleman who came to assist me.* Upon repeated observation he declared that its length was one-eighth of an inch, but to my eye it did not appear to be more than one-tenth of an inch, if so much. The angle which it formed with the wires being about 45 degrees, as near as the eye could judge, and lay from the stationary X to the revolving point D, when D was situated *between* X *and* Y *or the way the electrometer was revolving.*[*]

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* For the sake of those readers who are not conversant with this part of electricity, we will give a few words of explanation, showing the use of dividing the coil of wire. It was shown by Dr Watson and the other philosophers who made experiments in 1747 in order to discover the velocity of electricity by passing the fluid through two or three miles of wire, insulated by dry sticks set up on Hounslow Heath, that if they could perceive any perceptible time in the passage of the electric fluid, they would be enabled to settle the question of one or two fluids; for it was shown that if of one only, and the electricity started at one end as from the inside of the jar, to go to the other end or outside of the jar, the first spark would of course be seen at the end connected with the inside of the jar, and the last one with the one connected with the outside. But the case would be very different if it consisted of two fluids, for then the positive electricity would start from the inside of the jar at the very same instant that the negative electricity started from the outside of the jar, and the two, after travelling through half the length of the wire, would meet in the middle of the length of the wire;

Having failed in causing the electricity to strike the point Y when placed at the distance of half an inch from the point X, these two points were then placed nearer each other, the distance being only one-tenth of an inch, when I found that the discharge invariably struck the second point or Y; consequently, the velocity by such observation does not exceed 118752 miles in a second of time, for 18 inches×3·1416=

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and if the wire was divided in that place into two parts, a spark would be visible, and if the electric fluid took any time to travel through the conductors, the spark must necessarily be later in the middle than at the two ends; consequently, as the lateral deflection of the spark, caused by the time required for the electricity to pass through the wire, was only perceived when the coil was divided, it appears that the electricity consists of two electric fluids, and not one, which is the result at which Professor Wheatstone arrived. For the purpose of using the coil as one length, the connexions of the ends of the wire with the electrometer and jar, were different from those given in the plate. The end 5 of the coil was connected with the wire K or inside of the jar (a Lane's discharging electrometer being inserted between the coil and inside of the jar at O, as was done when the coil was divided), and the ends 2 and 4 joined together, and the end 3 connected with the points as shown in the plate, a wire being taken from I to the outside of the jar.

[*] By taking the length of the spark 1-10th of an inch, as the measuring unit of space in the circle of revolution; we have 56·5488 ×80×10=45239th of a second, for the time occupied during the transit of the point D over the unit of space; and consequently for the transmission of the fluid through the whole circuit, 1½ miles. Hence 45239×1·5=67858·5 miles per second would be the velocity.

But a still nearer approximation to the true velocity, would be obtained by assuming the perpendicular of a right angled triangle (whose base is the nearest distance between the points D and X, when the former is passing the latter, and whose hypothenuse is the length of the spark), as the measuring unit. Then, if c represent the circle of revolution in inches, r the revolutions of the point D in one second, p the perpendicular of the triangle, and l the length of the circuit in miles; we shall have $crpl = V$ the velocity.

In the experiment described in the text, if we assume the spark as 1-10th of an inch long, and that it inclined 45° towards the base of the triangle; that base would be=P=1-14th of an inch nearly, and 56·5488×80×14×1·5=95002 miles per second for the velocity.

56·5488, which multiplied by 10, because the points were the tenth part of an inch asunder, gives 565·4880; and this multiplied by 2 for half the distance of the points,* gives 1130·9760; which multiplied by 70, the number of revolutions made by the electrometer, gives 79168·32; and this multiplied by 1½, for the length of the wire, gives 118752 miles for the velocity of the electricity in a second of time. It is to be regretted that the propelling apparatus, from being made too weak, did not stand long enough to repeat this experiment more than twice or thrice, neither did it allow me an opportunity of observing what went on at the point C, namely, whether the electric fluid struck it before or after the direct passage at Z; but we may fairly presume that it took the points, or before C came up to Z, which point has always been granted to me, and if so, the objection which has been raised against the machine, that it is wholly influenced by the sparking distance, is untenable; for then, and in such case, the electricity would strike the point X, and not the point Y, which is contrary to the fact, and therefore the machine is influenced by the time required for the electricity to pass through the wire. For the more accurately observing the sparks at Z and X Y, holes were drilled in the upper part of the cast iron frame *directly* above these places, so that the eye might not observe them at an angle. The electrometer was driven by a wide leather strap, rubbed well with rosin to make it hold well on the wheel, which is preferable to a band. The instrument was not provided with an apparatus to reckon the velocity (though constructed for one), but as the other wheels were all cogged ones, and there was no reason to suppose that the band slipped in the least, the velocity given may be depended upon. The jar used was small, being only 10 inches high, and coated to 7 inches from the top; the diameter 5¼ inches. I have spoken of the electrical capacity of the jar which will probably not be understood by general readers without some explanation. It was very early discovered by electricians that of two Leyden jars of equal dimensions, the powers of them were not equal, but that one would melt twice the quantity of wire, or give twice the shock that another would, when both were *apparently* charged to the same extent. It was soon found that it depended upon the thickness of the glass, and that the thinnest glass took the greatest charge, and it has since been shown that the quantity of electric fluid, taken in by equal sized jars, is inversely as the thickness. It is the quantity of electric fluid retained by the jar that gives its electrical capacity.

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* It very naturally will be asked by the reader why "multiply by 2 for half the distance of the points?" To this we would answer, that as the electric fluid invariably strikes the nearest of two equally long and good conductors, the electric fluid in striking the point Y would do so when the point D had passed somewhat more than half way between the points X and Y.

Remarks on the above paper, which have occurred since the reading of it.

I was asked by a gentleman at Liverpool whether the Leyden jars were invariably marked by the passage of the fluid from one side to the other, and whether they had double lines. In answer I stated that five out of eight jars belonging to my battery were so marked, and that four of them were placed upon the table of the physical section, but that I had not observed it upon the surface of any other glass, except a very large and thick jar, which was borrowed where a great number of discharges had passed over and marked the surface, but that from being so close, the separation of the lines could scarcely be distinguished. Since that time I have made a few experiments in order to discover the cause of the lines upon the glass and their variable distances.

The jar marked No. 8 was exploded over the edge, and two zig-zag parallel lines were obtained at a considerable distance from each other, indeed the distance between them appears as great as between any of those before mentioned. The electrical capacity of this jar is 20½, and its weight 1lb. 14oz. No. 7 was then exploded over the edge, and the jar was marked, but very differently from any of the rest, for the spark divided itself as shown at fig. 13, Plate III.* A, being theinside branch and B the outside one. The lines are evidently double along the whole length, but at A and B the distance between them is very great, being greater than on any of the jars. It is very singular that in dividing on one side, the electric fluid should have divided on the other also. It is likewise curious that the lines should still be double when divided, which I take to be a further proof of the passage of the two electricities. Though the eye cannot distinguish a division throughout the whole length, nevertheless I think that we are allowed to infer it from the circumstance of its being visible through a part of the circuit. It will be asked by some, how comes it to pass, that of two electricities starting at the same time from the two ends of the circuit, and going in *opposite* directions, they should take the same line (however zig-zag) that the other electricity did? And secondly, why they should keep parallel, and not cross or *nearly touch*

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* This figure, and also figure 9, are the exact size of the original lines on the jars: but too large for both to appear in Plate II. Edit.

each other in some parts of the circuit? In answer to the first question I would say, that as the velocity of the electric fluid is infinitely greater than the velocity of any ponderable body known; if an opening or road is made by one discharge, the one coming after it will take that route in preference to any other, which it would be obliged to make for itself. Now it is well known that the air is a resisting medium, and if any proof were wanting of it, I might mention that in these spontaneous electrical discharges, the electric fluid (except in one instance) took the circuit of the glass along the turned over lip of the jar, in preference to the much nearer circuit through the air. Therefore, as soon as an opening has been made by one of the electricities, the other takes that instead of forcing one for itself, being in that light a conductor of electricity, and of two equally long conductors, electricity invariably takes that where it finds the least resistance. As to the second question, why the electricities should keep parallel and not touch each other, I am not prepared at present with an answer, but it appears to me to stand on the same footing as the great electro-dynamical principle, on which subject I am preparing a paper, and it would be trespassing too much upon it to say any thing further on the subject at present. The jar No. 5 was then exploded over the edge, but no mark was left upon it. It was exploded a second time with the same success. On examining the jar to ascertain the cause, I perceived that it was the new jar procured at the glass house in this place to replace one that was broken. Consequently, it appeared that it is not all kinds of glass that will mark with the electric fluid, and to discover to what it is due I took a large broken druggist's bottle, and caused the electricity to be exploded over the edge twice or thrice, by placing a wire on each side of the glass, close to it, but no mark was left upon the glass. But supposing that the failure might be attributed to the want of actual contact between the glass and the metal wire, I took two *pointed* slips of tin foil, and fixed them on the opposite sides of the glass by varnish, at the distance of an inch and a half from the edge and exploded, when a mark was left and the line was double both inside and outside, and they were parallel as usual. A common tumbler was then tried, and marks were left by the discharge. Afterwards a piece of crown or window glass was tried by the slips of tin foil, but I found it impossible to stain it by the fluid. It is therefore certain that the mark is caused by the glass and not the air, and is doubtless by a decomposition of the substance of the glass, and possibly in the flint glass by the reduction of the lead contained in it.

High Barnes, Sunderland.

V. *Letter to the Editor of the Annals of Electricity, Magnetism, Chemistry, &c., from* J. P. Simon, *M. D., Mem. Royal College of Surgeons, London; Professor of Nat. Phil., &c. &c.*

Sir,

I am truly sorry that, owing to unforeseen engagements, it has been absolutely out of my power to send you the communication on (as far as my practical observations enabled me to judge) the NON-IDENTITY of MAGNETISM and Common ELECTRICITY; which, as I had the pleasure of expressing to you, I wished so much to have inserted in your valuable scientific periodical. The idea with me is not of recent date, for it was about March, 1834, that I was led to a series of experiments which I established for the purpose of investigating whether the magnetic and electric fluids were to be, or could be, considered as "*identical.*" The first idea that struck me was the permanent situation which the common mariner's compass takes, and faithfully retains, unless some mechanical cause comes either to destroy or reverse its polarity. Can it be possible, I considered, that this magnetic needle is thus pointing to the north and south by a constant and undisturbed stream of electricity, and that magnetism and electricity are to be considered as one and the same identical fluid? Of the various facts which came under my observation I found most of them to be in favour of my objection, and not to admit of the identity of the two fluids. The following are the most striking, and to me, satisfactory. First, I observed that the magnetic fluid was not in most of them the least impeded by the presence of glass, which, (as well as all other known substances) it pervades with the greatest facility, and without, in the least, diminishing its intensity or power. I directed the magnetic stream from straight bars right through flat pieces of iron, holding the plate of iron in my left hand and the magnetic bar in my right, so that, had it been common electricity, I should have expected that it would have been conducted down from my right to my left side, and, consequently, unable to penetrate the iron wall, and thence perforate another wall of glass, and act powerfully on a freely suspended needle under the large glass receiver. I found that, on suspending a lady's steel busk by a delicate silk thread, it soon placed itself in the magnetic meridian and pointed permanently to the north and south, (similar results I believe have been observed by others, I mention it only as it is strongly in favour of my theory,) and that polarity was solely communicated to it by the influence of terrestrial magnetism. It is, I believe, a pretty well known fact that flame or the flames of candles will tend to, and do, disperse the electric fluid and prevent its accumulation; now magnetism can easily be induced into horse-shoe or bar magnets, by the usual means, (so satisfactorily described in your valuable Annals of Science;*) although the table on which the experiment is performed be filled with candle lights. If magnetism be the common electric fluid, the latter must then completely change its nature in order to accommodate itself to this new mode of fixing it into masses of steel which otherwise would answer only as an excellent conducting medium. The magnetic density, or intensity, *once* infused into sets of bars or compound artificial horse-shoe magnets, is not in the slightest degree affected by the state of the atmosphere, and if I touch with my naked hands a powerful permanent artificial magnet, I find no sensation whatever even by removing the keeper and taking hold of each pole in each hand, where I must feel certain that the magnetic influence must pass through my body, were I to think that magnetism and electricity were *one* and the *same* fluid; for I am certain that my thus touching the poles of the horse-shoe magnet does not in the least change its circumscribed magnetic scapement in the act of passing from one pole to the other. I admit, that, by voltaic electricity, considerable, and, indeed, indefinite, magnetic power can be induced into soft iron, surrounded by innumerable coils or helices of covered copper wires; but I do not think that voltaic electricity transforms itself into the new fluid called "magnetism," only as it pervades the various lengths or coils of wires, surrounding the piece of iron, by a chemical electric property inherent in itself, inducing magnetism, in proportion to the lengths of coils, into the centre of the helices, which, if filled by the most appropriate substance to accumulate and receive it, viz: soft iron; its magnetic intensity (or density according to the thickness or length of the wires) is developed to the highest degree, in proportion to the mass of iron and quantity of coils: the proportions of which are now pretty well ascertained, to obtain any required result without unnecessary loss of iron or wire.

I have now only time to send you the two drawings of my moveable electro-magnetic keeper, which I contrived in March, 1834; but which, from living out of London, I had no opportunity of publishing. It was my intention to have inserted it in the small work on electricity and electro-magnetism, &c.,

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* See Vol. 1, page 389.

which I had meant to publish; but who would venture such a thing now, when you have brought forth to the scientific world the means of communicating the mutual discoveries and observations of experimentalists at little trouble and no expense?

The first time I exhibited my electro-magnetic keeper was at my lectures at Plymouth and Devonport, and afterwards at Falmouth, where it was particularly noticed by the justly celebrated Robert Were Fox, Esq., author of so many valuable productions on the electricity of mines and on metals, &c., &c.; and by Sir Charles Lemon, who was also present, in December, 1835. These drawings were taken by, I believe, a Mr. Ash, schoolmaster, at Falmouth, and member of the Cornwall "*Polytechnic*" Society, of which I have the honour of being a member. This instrument, in the investigation of that particular branch of science, electro-magnetism, will at once appear particularly useful; for, with such an apparatus, the electro-magnetic spark can easily be obtained from any permanent, or electro-magnets, by presenting it to them, as in fig. 7 and 8 Plate 1, and thus at once ascertain the force of the magnet to which it is presented. Fig. 7 is a flat and full view of the electro-magnet; the iron of which is an inch and a quarter square, having 450 yards of coils of copper wires (No. 13.) on each pole.* A single cylindrical STURGEON's battery measuring about 4½ inches in diameter by 10½ high, induces in it a magnetic power of upwards of four hundred pounds weight. The terminating wires *a, a,* of the coils communicating with the cylindrical galvanic battery as shown in the figure 7. The wooden stand on which the electro-magnet rests in a horizontal position is represented by 4, 4, 4, 4. The revolving keeper round which 120 yards of wire are coiled, 5, 5. The terminating wires are made to pass through a brass tube of about ½ inch (as I have not the apparatus before me) in diameter, 6, 6, 6; a hole is perforated right through the brass pulley 7, and through the back of the keeper, 8, 8. The wheel is represented by 9, 9, and the string is seen in the side view of the apparatus, fig. 8, which, I hope, will at once appear satisfactory and intelligible. 10, fig. 8, the wooden stand through which the hollow axle-tree or tube passes and turns through a collar of gun metal. A brass ferrule *f*, is fixed on a piece of nicely turned ebony, having a hole in the middle to allow the centre communicating wire 12, to pass through, and a smaller hole immediately (eccentric) above it, to allow a passage for the last communicating wire, which is soldered to the copper ferrule 13, to ensure perfect contact. This part of the apparatus is the same as the Saxtonion. 14 is the steel disc, and 15 the diamond-like steel point. 16, the ebony cup to hold the quicksilver. 17, a thumb screw to raise or depress the cups according as is necessary. 18, 19, the cups of the battery 20. The whole machine will be at once understood. But now, Mr. Editor, I beg to draw your attention, and that of your numerous scientific readers, to the valuable and important improvement I made in the metallic disc and in the small lancet-like blade. This arrangement I made also in March, 1834, and I was surprised to find that although I had exhibited it for so long a time to my classes that it had not reached the Adelaide Gallery of Practical Science, where the old copper disc and blade are still in use. I have not published it in any scientific journal, for the reason above assigned of my intention to publish a small work on the various branches of physical science on which I lecture. Many thousands, however, have seen it, and it was complimented by the Rev. Mr. Scoresby of Exeter, as being a great improvement in the apparatus. Had Mr. Clarke, "*the magnetician*," known that fact, he might have saved himself the trouble of showing to your readers the disadvantages of the copper metallic disc and copper blade still in use in the Adelaide Gallery of Practical Science, and where both the blade and copper disc *scatter* the mercury about as stated in your Annals, page 147, No. 2, January, 1837, and this improvement consists in substituting a polished steel disc, extremely fine and soft, and the diamond or blade of the same metal. This, Mr. Editor, as you will readily conceive, having no affinity for mercury, passes *freely* through that metal without disturbing it in the least. It is, I presume, almost as good a conductor as the copper disc and blade; indeed I have no reason to think it inferior. True it is that the spark does not appear so large or intense; but this must be attributed to the metallic steel blade not carrying with it, as in the case of the copper, any mercury out of the cups, part of which may be considered as entering into fusion. Here the spark is not so large; but it is genuine and beautiful, and my test for ascertaining that it is not an inferior conductor of the electro-magnetic fluid is, that it reddens the same quantity of fine platina wire as though the copper disc and blade were used. The decomposition of water, &c., is also as quickly effected by it.

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* From the circumstance of there being no representation of coil wire in the drawing which was sent to us, we are led to suppose that the wire which covers the magnet is enveloped in a silken or other case. EDIT.

Preparing for my lectures in this place, I cannot say any more until some future opportunity. I have, I think, stated my general ideas on this subject; and if I am fortunate enough to be in time for you to give it a place, I will deem it a favour, intended as it is to throw some useful hints upon a subject of such vital interest.

I have the honour to remain,
Mr. Editor,
Your very obedient and humble servant,
J. P. SIMON.

1, *Cisternfield Place, Exeter,*
September 23, 1837.

VI. *Influence of Electrical Action on Clay.* By ROBERT WERE FOX, ESQ.[*] *Read before the London Electrical Society, at the Meeting of the* 16*th December.*

At the annual meeting of the Cornwall Polytechnic Soceity, I exhibited specimens of dry clay which have a *laminated* appearance like clay-slate, in consequence of its having been exposed to long continued voltaic action when in a moist state. In two instances I employed the clay to divide earthenware vessels into compartments, or water-tight cells, so that the clay formed, as it were, a wall between them. Into one of these cells I put a piece of the native bi-sulphuret of copper, and a solution of the sulphate of zinc; and in the other cell a piece of zinc together with water acidulated by sulphuric acid, the connexion between the copper ore and zinc having been completed by means of a copper wire which was passed over the wall of clay.[†] The arrangements in the other vessel were nearly the same, except that iron was substituted for zinc in the acidulated water. Thus circumstanced, the vessels were allowed to remain undisturbed for a considerable time, till the liquid was evaporated, and the clay became dry. The latter then presented a *laminated* structure, the laminæ being atright angles to the direction of the voltaic action, *i. e.* to the plane of the circuit; with one *principal division or joint,* in each case, separating the clay into *distant* portions parallel to the laminæ. These results seem to have a direct bearing, not only on the lamination observable in many sedimentary rocks, but also on the existence of metalliferous deposits in some rocks, and not in others, and in some beds of clay-slate, rather than in other contiguous beds of the same substance, in consequence, as I have assumed,* of their having been in *different electrical states;* for that the *divided portions of clay* in the experiments in question were in *opposite states of electricity* can scarcely be doubted: and I conceive that the electrical theory of mineral veins which I have ventured to put forth,† derives strong confirmation from these results.

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[*] Communicated by the author.

This paper was accompanied with three specimens of clay: one of which "was kept in a moist state for a considerable time, but *not* exposed to voltaic action, and therefore not laminated." Another specimen had "originally been moistened by a solution of sulphate of copper, and placed for some months between copper pyrites and zinc, forming a voltaic circuit." This specimen, when broken into fragments, was found to contain a few scattered specks of the oxide of copper.

The third specimen exhibits a beautifully laminated structure, which it has acquired by voltaic action, as described in this paper.

These specimens were placed on the table of the Electrical Society, and excited much interest amongst those who examined them. EDIT.

[†] A figure of Mr. Fox's apparatus will be seen in Plate V. vol. I. and described at page 133. The specimen of clay first mentioned in the preceding note, we have placed in a voltaic circuit similar to that described by Mr. Fox. Our readers may expect to hear of the result. EDIT.

It seems reasonable to believe that the structure and relative characters of coal beds are due to the same agency; and it is not improbable that the very thin layers of shale which often sub-divide given coal beds into parallel portions, may have been so arranged by electrical action.

Many of the least homogeneous sedimentary rocks seem to have a greater tendency to exhibit a *laminated* structure than those which are more chemical in their composition; such as lime-stone. In the former case, it would seem as though a more external or general influence had prevailed, and in the latter, a more internal or atomic attraction.

Before I conclude, it may perhaps be well for me to state, that having taken masses of clay moistened by a solution of sulphate of copper, and exposed them for five or six months to weak voltaic action, they were, on examination, found to contain numerous insulated portions of metallic copper, also the red oxide and carbonate of that metal. They, moreover, included veins of the same substances, some of which covered a surface, or rather filled a crack, of more than a square inch in extent, although not much thicker than paper.

These results seem to be analogous to those communicated at Liverpool to the chemical section of the British Association by Golding Bird, and they are also in accordance with the views I have stated in the Polytechnic Report, p. 117, of the manner in which electricity seems to have operated in causing metallic deposits in the earth; and likewise with an experiment which I made about fifteen months ago, in which a bladder containing zinc and acidulated water, when plunged into a solution of sulphate of copper, was coated on its *outer surface* by metallic copper. See Polytechnic Report for 1836, p. 135.

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* See Royal Cornwall Polytechnic Society's Fourth Report, 1836, pp. 110, 113, and 114: sold by Trathan, Falmouth; and Simpkin and Marshall, Stationers' Court, London.

† Mr. Fox's theory will soon be placed before our readers. EDIT.

VII. *Memoir on the relative measure of thermo-electric and hydro-electric sources, and on the quantities of electricity which are necessary for the chemical decomposition of* 1 *gramme of water, or to give stronger or weaker commotions under certain circumstances; by* M. POUILLET.*

(Extract by the Author.)

1. *Comparison of the thermo and hydro-electric sources.* All thermo-electric sources may be compared among themselves by laws which were developed in a memoir presented to the Academy in 1831.

All hydro-electric sources may be compared among themselves by laws developed in a memoir which was presented to the Academy 20 Feb. 1837. (See the "*Compte rendu*" No. 8.)†

There remained to determine the relative intensities of these two electric sources so different in their nature and properties, although subservient to the same laws; to arrive at which, the following process has been made use of. A platinum wire of ·144 of a millimetre in diameter, and 200 metres long, with a single end, was placed in a convenient manner; the resistance of Wollaston's ordinary pile of 12 pairs was determined, and its current made to pass over the pyrometric compass, (see the "Compte rendu des Séances de l'Academie" 26 Dec. 1836) and over a sufficient length of platinum wire to reduce the deviation of the compass to 16°. The total length of this circuit was then 180 metres of platinum wire. A thermo-electric current, produced by a bismuth and copper source, was afterwards passed over the same compass, traversing (the compass included) a length of 21 metres of copper wire, 1 millimetre in diameter; the deviation was likewise 16° for a difference in temperature of 42°·4. The conductibility of the copper wire of this circuit was 6·5 with regard to that of the platinum. From these data it is easy to conclude that the intensity of the hydro-electric pile is equal to 113924 times that of the element of bismuth and copper, having a difference in temperature of 1°, and a circuit of copper wire 21 metres long and 1 millimetre in diameter.

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* Translated by Mr. J. H. Lang; from the Comptes Rendus.

† This memoir will appear in our next number. EDIT.

This process may be employed in all cases, only it will give as many different results as different sources are made use of.

Thus, all electric sources may be brought to one unity, and it is shown how this unity may itself be related to the magnetic intensity of the earth, and how it may become a unity to measure electric sources as the degrees of the thermometer for measuring temperatures.

2. *Relative conductibilities of liquids and metals.* The worst metallic conductors have so very great a conductibility in proportion to that of the best liquid conductors that there has been no exact comparison made of them, notwithstanding that this comparison is one of the most essential elements of the theory of electricity. This research has been made in the following manner: It was first demonstrated by a great number of experiments—that for liquids as well as metals, the conductibility is a proportion inverse to the length and direct to the section, provided that the length of the cylindrical column of the liquid be at least equal to five or six times its section; from which it results that the conductibility of liquids is rigorously comparable with that of metals. This principle being set at rest, the saturated solution of sulphate of copper at a temperature of 15° or 16° was chosen from among the liquids to be compared to a long platinum wire 200 metres in length. For which purpose, the current of a pile was made to traverse the pyrometric compass, and a column of sulphate of copper 1 metre long and 20 millimetres in diameter; the deviation by the compass was 22°. The same current was then made to traverse the compass, and a length of platinum wire, increasing or decreasing until the deviation was 22°, and it was then found that 132 metres of platinum wire were obliged to be placed in the circuit.

From these data, it is easy to conclude that the conductibility of platinum is

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times greater than that of the saturated solution of sulphate of copper; and with regard to this solution the conductibility of metallic copper is, consequently, more than 16 millions, and that of palladium more than 30 millions.

The conductibility diminishes in proportion as the solution is further removed from saturation; but it does not diminish in a very rapid manner, as will be seen by the following table.

Solution saturated with sulphate of copper conductibility	1·00
——— extended by 1 volume of water	0·64
——————————2	0·44
——————————4	0·31

Still taking for unity the solution saturated with sulphate of copper we have

For the solution saturated with sulphate of zinc	0·417
— pure water	0·0025
——————— with $\frac{1}{10,000}$ nitric acid	0·015

The above results are those which were obtained by making the contacts with the copper in the sulphate of copper, with the zinc in the sulphate of zinc, and with the platinum in the pure or acidulated water.

3. *Quantity of electricity necessary to decompose* 1 *gramme of water.* In a current of invariable intensity, the quantities of electricity which circulate or move are evidently proportional to the duration of the passage of the current; that is to say, that in double the time there will be double the quantity of electricity put in motion, &c. To know, in such a current, if the quantity of electricity be reduced to half when the electro-magnetic intensity is itself reduced to half, the following means have been adopted; a mechanism has been used capable of opening and shutting the circuit a great many times in a second, and consequently fit to interrupt or permit the passage of the current: the duration of the interruption was exactly equal to that of the passage. When the interruptions were not very numerous, the needle of the compass oscillated; but as soon as they were increased to 140 or 150 in a second the needle was steady as if under the influence of a continued current, and it showed an intensity exactly equal to half that of the primitive; from this time it remained immoveable, and continued to show an intensity half of what the number of interruptions were, even when it was raised to 3000 in a second. But when, for example, there were 1000 contacts and 1000 interruptions in a second, it might be perceived that the electricity which passed during each contact was divided into two equal parts, of which one would be put in reserve to pass during the following interruption; we should thus have a continued current, in which only half of the electricity passing in the primitive current would pass in a second, and as the intensity shown by the compass is only half of that of the primitive, we may admit that the quantity of the electricity is proportional to the intensity of the current. Thus, when we have two currents, whatever may be their sources, it is sufficient, to observe their relative intensi-

ties, to have a measure of the relative quantities of electricity which constitute them, and since the same electric source gives currents whose intensities are in a proportion inverse to the length of the circuit and direct to the section and conductibility, there still results that the same source gives quantities of electricity continually varying, and varying according to these laws.

To compare now the intensities or *tensions* of the sources with themselves, *equal sources* or *equal tensions* are those which in the same circuit produce currents of the same intensity, and one source will have an intensity double or triple that of another, when in the same circuit it will produce currents of double or triple the intensity, &c. However extensive, it always acts over the whole of the circuit, including the source and its resistance, which ought to be estimated by the laws of conductibility.

It results from these definitions:

1st. That the tension of any source is independent of the size of the elements which compose it.

2d. In any pile, the tension is always equal to the sum of the tensions of all the sources or of all the elements that compose it.

In short, there results, that if for the unity of tension, the tension of the thermo-electric source bismuth and copper at a difference of temperature of 100° be taken, and for the unity of quantity the quantity of electricity which this source gives in a minute, in a circuit of 20 metres of copper wire whose section and conductibility are taken for unity, the tension T of any source and quantity Q of electricity which this source sets in motion in a minute, will be given by the two following equations.

$$T = \frac{20}{L'} \frac{1}{b} \frac{\sin D'}{\sin D}$$

$$Q = \frac{1}{b} \frac{\sin D'}{\sin D}$$

D is the deviation produced on any compass by the current that is taken for unity.

D′ is the deviation produced on another compass by the current that is required to be estimated, and the relation of the sensibility of this compass to the preceding one is represented by $\frac{1}{b}$.

L′ is the length in metres of the circuit traversed by the source whose tension is required to be estimated, the circuit being formed of copper wire of the section 1 and conductibility 1.

That set at rest, in order to know the quantity of electricity necessary to decompose chemically 1 gramme of water, there remains to show that this quantity is constant, that is to say independent of the intensity of the current, already established for the decomposition of nitrate of copper by M. Becquerel (see "Comptes Rendus" 9 January, 1837, page 40)* is confirmed for water by different series of experiments analogous to that which is related in the following table.

Table of a series of experiments on the decomposition of water more or less acidulated.

No. of the experiment.	Nature of the Liquid.	Metal which forms the Poles.		Number of seconds to obtain two cubed centimetres of hydrogen	Deviation of the Compass needle	Sine of the deviation or Intensity	Product of the Intensity by the Time.
		Positive pole.	Negative				
1	Distilled water With sulphuric acid.	platinum	platinum	498″	5°50′	0·1016	50·60
2		do	do	510	5°40	0·0987	50·34
3	The same liquid increased by a volume of distilled water.	do	do	725	4·00	0·0697	50·63
4		do	do	728	4·00	0·0697	50·74
5		do	do	919	3·10	0·0552	50·73
6	Common water with sulphuric acid.	do	do	417	6·50	0·1190	49·62
7		do	do	423	6·45	0·1175	49·70
8		copper	do	251	11·20	0·1965	49·32
9		do	do	247	11·30	0·1994	49·25
10		do	do	247	11·30	0·1994	49·25
11		zinc	do	239	12·00	0·2080	49·71
12		do	do	258	11·00	0·1908	49·21
13		platinum	do	684	4·10	0·0724	49·50
14	Sulphuric acid increased.	do	do	77	40·00	0·6428	49·50

This table shows, in reality, that under very different circumstances, and for intensities which vary from 1 to 12, the product of the intensity by the time necessary to obtain two cubed centimetres of hydrogen, is evidently constant, which proves that the quantity of electricity that has produced this effect is absolutely the same. To determine the value of this quantity Q, we have

$$\frac{1}{b} = 17{\cdot}3;\ \sin n' = 0{\cdot}1001;\ \sin n = 0{\cdot}6510;$$

which gives $Q = 2{\cdot}665$

and as the experiment lasted 8′—20″, this quantity becomes

22·208,

taking for unity what passes in a minute.

We have for two centimetres cube of hydrogen, and consequently to decompose 1 gramme of water, a quantity of electricity expressed by

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* See Becquerel's paper, page 398, vol. I. of these Annals. Edit.

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will be required, that is to say, a quantity which is 13787 times greater than that which passes in a minute through a circuit of bismuth and copper, 20 metres long and one millimetre diameter of copper wire, with a difference in the temperature of 100 at the solderings.

This number is sufficient to express the quantities of electricity necessary to make any other chemical decomposition, since by the very remarkable law discovered by M. Faraday, and confirmed by M. Becquerel, the numbers which represent the chemical equivalents of different substances, are also those which represent the ponderable quantities of these substances which are decomposed by a like current, and consequently by a like quantity of electricity.

4. *Electric intensities necessary to produce stronger or weaker commotions.* The physiologic effects have been measured with a very sensible compass, whose multiplier had 240 turns, the ellipse of the circumferences nearest the needle, having a large axis of 10 centimetres, and a small one of 2 centimetres.

It was stated, at first, that the current which passes from one hand to the other, they being moistened and dipped in mercury, is weakened as much as if it had traversed 11 leagues of copper wire one millimetre in diameter, and that the current which passes from one finger to another in the same hand, is weakened as much as if it had traversed 77 leagues of the same copper wire, the fingers being moistened and immersed in mercury as far as half or a third of the first joint.

We afterwards compared the electric intensities which are necessary to produce the weakest perceptible commotions, and the most insupportable ones, and have found that these intensities are as 1 to 18 or 1 to 20, the communications remaining the same.

The characters of these commotions, their increasing intensity from the first joint to the articulation of the wrist, and even to the elbow, appear easy to explain by the considerations only of the conductibility, and of the division of the electricity into the different organic conductors.

All these observations lead to the conclusion that the electric fluid shows its effects, not in proportion to the sum of the actions that it exercises, but in proportion to the intensity of the individual actions that it exerts over each of the fibres that are destined to receive or transmit the impressions that it may produce; and that in this respect it acts in a manner analogous to light.

5. *Examination of the mechanic conditions of the motion of the electricity, and the general principle which results from it.* From the impossibility of entering here into the examination of these conditions, we shall limit ourselves to giving the enunciations of the four propositions which express the general principle.

1st *Proposition.* The current is not produced in a constant manner, but it is produced by interruptions, whose duration, always excessively small, is however dependant on the tension, source, length, the section, and conductibility of the circuit.

2d *Proposition.* Each interruption is composed of two periods, the one which may be called the period of decomposition or polarization; the other which may be called the period of recomposition or depolarization.

3d *Proposition.* The polarization is accomplished in a given and variable time, which is always excessively small, and it ought to be accomplished over the whole chain or throughout the whole extent of the circuit, before the depolarization can take place; this polarization seems to be a kind of decomposition by influence which operates upon each particle, or more generally upon each electric element.

The duration of the polarization is proportional to the length of the circuit, and to the quantity of polarized fluids when the polarizing force remains the same; but it is in an inverse proportion to the electric conductibility of the circuit and is independent of the magnitude of its section.

4th *Proposition.* The recomposition is instantaneous and simultaneous, that is to say, that it is accomplished in a time, inappreciable with regard to that which is required for the polarization or decomposition of fluids, and that it is accomplished at the same time or simultaneously in all the electric elements of the circuit which have previously been polarized.

As soon as the recomposition has taken place, the same cause subsisting, polarization recommences over all the elements of the circuit; then, when it has every where acquired an equal intensity, suitable to the particular conditions which belong to the source and to the circuit, it is followed by a new composition, and so on.

VIII. *Electrical Society of London.*

The first evening meeting for the season took place on Saturday, October 7, in the Theatre of the Gallery of Practical Science, Adelaide Street, Strand:—

Previous to the chair being taken, Mr. Bradley, the Superintendent of the Society for the illustration of Practical Science, exhibited a thermo-electric battery, which has been recently constructed for the Gallery, under Mr. Bradley's superintendance. It consists of fifty-six pairs of elements, placed vertically, consolidated with plaster of Paris. Mr. B. stated that with a difference of temperature of 19°, sparks had been elicited and a permanent deflection of the needle caused. This battery is at present exhibited in the Gallery.

On the chair being taken, the following Report of the Committee was read by the Assistant Secretary:—

REPORT.

Your Committee, in pursuance of your instructions, have caused a Report to be printed and circulated; which report, dated 4th. September, having been sent to each member, your Committee do not consider it necessary further to allude to it.* Your Committee have met from time to time during the recess, and have great pleasure in announcing, that considerable increase has already been made in number of the members of this society; and your Committee suggest, that it would materially assist the objects for which the society was originally instituted, if the number of resident members was increased to 100, previous to the election of officers. Your Committee have secured to the society, the valuable services of Mr Leithead, who, at the request of the Committee, has kindly undertaken the arduous office of honorary secretary.

The printed report already circulated, has acquainted you, that the future evening meetings will be held in this Theatre, and the Committee cannot but congratulate the members upon the liberality evinced by the council of the Society for the Illustration of Practical Science, in thus affording to this society such accommodation. For the convenience of the members, a card for the admission of visitors has been engraved and issued to the members. Should any member require a further number, the same will be forwarded on application to the secretary.

Your Committee have taken into consideration the best mode of opening the proceedings of the session; many propositions were submitted to the Committee, but they have decided on requesting Mr. W. Sturgeon, as the first member who acted as chairman when the society was originally proposed, to state the objects and character of the Electrical Society in an opening address, which will be delivered by that gentleman this evening. Your Committee cannot conclude this report without congratulating the members on the prospects of the society. The subscriptions already exceed the amount of expenditure; at the same time, it is to the increased energies of the members, that the Committee look forward for the means of carrying out the objects for which the society was originally formed. The Committee propose the following rules and regulations for the consideration of the society, and if approved, for their adoption.

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* The Report is inserted in the first volume of these Annals, page 416. Edit.

The rules and regulations are essentially the same as those stated in the Annals, page 416, vol. I.; with the exception that the forming of a permanent council should be delayed until at least one hundred *resident* members had been enrolled.

After the Report, &c., having been confirmed and adopted, Mr. Sturgeon read the following Address, which has since been printed by the Society, and will form part of its transactions.—

Address, delivered by W. STURGEON, ESQ., *Lecturer on Experimental Philosophy at the Honourable East India Company's Military Seminary Addiscombe, &c. &c. at a general meeting of the London Electrical Society, held in the Theatre of the Gallery for the illustration of Practical Science, Adelaide Street, on Saturday, October 7th,* 1837.

Gentlemen,

The Report of your Committee having been read by the Secretary this evening, I am now called upon to explain to you the nature of this Society, and the objects for which it has been formed. I cannot proceed to address you, however, without, in the first place, expressing my entire concurrence in every particular embraced in the Report, with the solitary exception of the appointment of myself for the discharge of this important duty, being perfectly convinced that some other member, more efficient than I am, might certainly have been named. The choice, I am persuaded, cannot be thought to have arisen from any consideration of ability: I am more willing to believe that it is an act of polite courtesy on the part of your Committee, occasioned, perhaps, by the circumstance of my taking the chair at the meeting of a few friends to science, when the Electrical Society was first formed. But it will be remembered, by those gentlemen who were present on that occasion, that, notwithstanding the impressions which I experienced of the honour they were pleased to confer upon me, it was with extreme reluctance that I assumed an office, to fulfil the duties of which, others about me were more amply qualified. But the laudable motives from which this Society was originally instituted must ever claim the respect and regard of every friend to science; and the desire which I have, as an individual member, of being instrumental in promoting its present interests and future prosperity, will, at all times, place my humble services at the command of your Committee, in any way they may deem them most useful; and I should wish it to be distinctly understood, by this assembly, that it is from a full conviction that the cultivation of electricity will ultimately confer the most important benefits on mankind, and from a firm belief that its data will become more numerous and exact, and, consequently, its advancement more rapid, by a co-operation of experimentalists, than by the insulated position in which they have hitherto been permitted to labour, that I now appear before you as an advocate for the interests and success of the *London Electrical Society.*

From a consideration that the greater number of this assembly are probably acquainted with the progress which electricity has hitherto made, it is not my intention to attempt, in this address, to pourtray even a brief outline of the science; notwithstanding, I am inclined to believe, there are many who might suppose that, on an occasion like the present, a recital of some of the most prominent discoveries and events which have marked the progress of electricity, could neither be conveniently dispensed with, nor with propriety omitted. I am well aware, however, that the cultivators of electricity, who now hear me, have no need of my services to bring to their recollection the splendid discoveries which are recorded in the history of the science. Those are events already indelibly stamped on their minds; they are stimulants to renewed experimental enterprise, and are land-marks by which they are guided in the pursuit.

But it may be said, if those distinguished events which have marked the respective eras in the progress of the science be looked upon as shining beacons to the experienced electrician, it is possible, that even a brief view of their splendour might incite the amateur to renewed exertions, and lead him to discoveries still more important than any that have hitherto been recorded. Moreover, I should labour under no unpleasant apprehensions of being tedious to our veteran electricians by a brief retrospection of some of the principal discoveries which have been made in their favourite science; because, I can easily imagine, that their ardour for fresh enquiries becomes rekindled, and reverberates with new energy, at every recital of those eventful periods which are so prominently arranged in the same path of discovery which they themselves are now successfully pursuing. But, however pleasing and useful might be the recital, I forbear detaining you with a detail of facts so abundantly recorded in almost every treatise on electricity, and so easily accessible to any one wishing to become acquainted with them.

No one, I am persuaded, can contemplate the multitude of facts which have been developed by the industry of experimentalists; the splendour and beauty of those facts; the simplicity of their production; the harmony of their display; their analogy to some of the most mysterious operations of nature; and their general tendency to improve the condition of mankind, and inspire the most sublime ideas that the mind is susceptible of enjoying; without entertaining the most flattering hopes of the success of a well conducted Electrical Society. Indeed, whoever takes a general survey of electricity, with its thousands of imposing phenomena, ramifying through an almost endless variety of nature's productions, and forming the most important experimental science ever cultivated by man, can feel no hesitation in acknowledging the probable utility, at least, if not the absolute indispensability, of a society wholly devoted to its cultivation; and it is hardly possible that he can quit the contemplation without experiencing feelings of astonishment and regret that such a society had not been formed and matured many years ago.

The last forty years have been more productive of electrical discovery than all the previous centuries embraced in the history of the science; yet even seventy years ago, when electricity had not assumed half the importance it now presents, the eminent Priestly saw the necessity of Electrical Societies, as will be understood by the following passage from the preface to his History of Electricity. "The business of philosophy is so multiplied, that all the books of general philosophical transactions cannot be purchased by many persons, or read by any person. It is high time to subdivide the business, that every man may have an opportunity of seeing every thing that relates to his own favourite pursuit; and all the various branches of philosophy would find their account in this amicable separation. Let the youngest daughter of the sciences set the example to the rest, and show that she thinks herself considerable enough to make her appearance in the world without the company of her sisters."

These, gentlemen, are the words of the first electrician of the age, written in the year 1767; long before Galvanism, Electro-magnetism, Thermo-electricity, or Magnetic-electricity were known. If, at that time, Electricity, the then youngest daughter of science, was considered of sufficient importance to make her appearance in the world alone, what would the learned Priestly have thought had he lived to see her make such progress as she has since done; to see her *not* the youngest daughter of science, but a parent of other sciences: and still without being deemed worthy of a separate establishment amongst their temples?

When speaking of incorporated societies, Dr. Priestly says, "I by no means disapprove of large, general, and incorporated societies. They have their peculiar uses; but we see by experience that they are apt to grow too large, and their forms are too slow for the despatch of the minutiae of business in the present state of philosophy."

This passage probably alludes to the Royal Society; a society for which I shall ever entertain the highest degree of respect, and which will, I hope, always maintain the elevated rank it has hitherto held in this, and, I believe, in every other civilized country.

Any one acquainted with the rise and progress of institutions must be very well aware that, however formidable or imposing may be their present aspect, most philosophical societies, of any note, have commenced under very humble circumstances, generally, by the uniting of a few friends who were favourable to scientific pursuits. It is well known, for instance, that the Royal Society emanated from the praiseworthy exertions of a few scientific individuals during the time of the civil wars. Its members rapidly increased in number, and no society has contributed more to the advancement of natural and experimental knowledge, than the Royal Society has done, by the publication of its memoirs, under the title of *Philosophical Transactions.* But it must be acknowledged, that a work embracing such a diversity of subjects, as, from the nature and constitution of the Royal Society, are necessarily printed in its transactions, is very far from being well adapted for the perusal of those experimentalists, whose enquiries are directed to one particular branch of science. The same remarks will apply to the transactions of every society whose object it is to promote, indiscriminately, the advancement of all branches of natural knowledge, and they are equally applicable to every periodical whose contents are of a miscellaneous character.

It is true the philosophical transactions are enriched by communications from men distinguished in almost every department of science; and, from the variety of topics they embrace, may be consulted with nearly equal advantage by the astronomer, the geometer, the natural historian, the anatomist, the physiologist, the chemist, and the electrician; but the astronomer has seldom a desire to be impeded in his favourite pursuit by having to traverse the memoirs of the chemist, or the electrician; nor has the physiologist any particular wish to read, much *less* to purchase, the works of either of them. It is thus that valuable papers, on particular subjects, are entirely lost, to those who would take the greatest interest in perusing them; and facts, which are now re-appearing as new discoveries, have long lain smothering amongst heaps of records on topics entirely foreign to them.

Sensible of these impediments to the progress of their individual sciences, the Astronomers, Geologists, Horticulturists, Engineers, Entomologists, and some other classes of scientific men, have found it advisable to form themselves into distinct societies for the propagation of their respective branches of research. And it is really surprising that electricians, the importance of whose labours is second to none, should so long have permitted themselves to remain disunited and insulated from each other. Electricity, however, has hitherto been left to struggle through the exertions of individuals, many of whom, having fettered themselves with certain hypotheses, appear more intent on advocating particular theories than in furthering the cause of the science.

It is possible, I imagine, that some may be of opinion that, as the present age has produced so many experimentalists of acknowledged ability, electricity may be safely left in their hands. I am myself of that opinion; and it is one of the principal objects of this Society to place it in their hands *unitedly*, and as a body of electricians. The Committee, therefore, invite every electrician to join this Society, being confident that, by their united labours, electricity will speedily be placed on the same footing as the most acknowledged exact science. But, if the cultivation of electricity were to be still left to the caprice of individuals, it is not likely that their theoretical opinions would soon be reconciled to each other. Many of them would be still left to flutter on the wings of vain hope, and ponder away their valuable time in whimsical hypotheses which have no reality in nature. It would be easy to swell out this address with the hypothetical incongruities of the present day; but I forbear. Their only use is to show how cautious we ought to be in receiving as axioms the opinions of individuals, however eminent they may be esteemed for their scientific attainments.

Electricity is yet, and must for some time remain, a science of experiment and observation; and the advice of Dr. Franklin, to one who asked him for his opinion as to some hypothesis which he was anxious to advocate, cannot be too literally followed. "I would recommend you," said the philosopher, "to employ your time rather in making experiments, than in making hypotheses and forming imaginary systems, which we are all apt to please ourselves with until some experiment comes and unluckily destroys all our expectations."

The importance of Electricity has invariably been a subject of much comment at the annual meetings of the British Association; and many valuable papers, on certain branches of electrical science, have appeared in the volumes of its transactions: but it has not unfrequently happened that matter of the greatest importance to the general interests of the science, which has been elicited at the discussions, has been suffered to be entirely lost, excepting to those few who had the good fortune to be present. Every one who has attended the meetings must acknowledge this fact. Indeed, even those persons who have been fortunate enough to get their papers read, or apparatus exhibited, have usually been so hurried, from a want of sufficient time being allowed, to enable them to proceed in the calm uninterrupted manner essential to give full effect to their performances, that the principal part of the object for bringing them forward has been entirely defeated; while important information on certain points, which might probably have been developed by discussion, has been absolutely stifled, by the pressure of other matter, possessing, perhaps, equal claim to attention, at the same physical section.

It must, however, be acknowledged, that much praise is due to the projectors of the British Association. Its migratory meetings are well calculated to awaken the latent faculties of the thousands who attend them, and to inspire a spirit of scientific emulation throughout every province in the land. But its business is multifarious; and its transactions, which appear annually, are necessarily of a miscellaneous character. Moreover, much that is valuable, which transpires at the discussions, never appears in them; and which, if ever published at all, is by means of other journals. But I shall not dwell any longer on the proceedings of other societies, each of which has its particular uses, and is productive of more or less good to the general interests and welfare of mankind. I have mentioned these for the purpose of showing that their province is to propagate natural knowledge generally, and that they are not devoted to the culture of any particular branch.

This society has been formed for the purpose of offering to the cultivators of electrical science an opportunity of avoiding the various impediments which have hitherto been placed in the way of their own particular study. The Committee will afford every facility for the publication of their discoveries, as well as to their descriptions of new instruments, in the volumes of the transactions of the Society, which will be illustrated with appropriate diagrams, and devoted to no other subject than the various branches of Electricity. By thus encouraging the cultivation of the science, the Committee feel an entire persuasion that the number of members forming this society will speedily increase, and the progress of Electricity will thereby be rapidly promoted.

It has been stated, and I am aware with some truth, that, hitherto, the management of the different philosophical societies of London has generally fallen on the same individuals. That the prominent and most efficient members of the Royal Society are also those of the Astronomical, Geological, and other Societies, and that it is impossible these gentlemen can devote any more of their valuable time to the services of any other, however important its object, or however much it may be required.

There is no one would more deeply regret than myself our being deprived of the co-operation of such distinguished individuals as now adorn the councils of those societies; and it is with a view of securing to the Electrical Society the advantages derivable from the services of those eminent men, that the Committee have advised the number of resident members to be augmented to one hundred before its officers are elected; and, sooner than the Society should lose any opportunity of securing the assistance of such eminent talent, I should strongly recommend even that number to be increased before its council is formed. But should we even not be fortunate enough to enrol such names in the lists of our council, there can be no fear as to the general result. The number of individuals previously unknown in the annals of science, who have, within the last few years, devoted their time as well as pecuniary means to the cultivation of electricity, affords this Society every prospect of receiving their assistance; whilst, on its part, it will become a parent to foster and cherish their investigations; a grand storehouse in which they may repose the rich productions of their labours, and a temple for their kindred spirits' resort.

I am well aware that many are of opinion, that those alone who are deeply skilled in experimental investigation can really be useful to science. Such an idea is as groundless as it is detrimental to the progress of any particular branch. Let no one imagine that he cannot render science a service. I have already stated that electricity is a science of experiment and observation; and, therefore, this Society cannot receive greater assistance than by the communication of such facts as, from time to time, come under the notice of its members. The Secretary will at all times be happy to receive such communications, and lay the same before the Committee; and should any member require information as to the proceedings of the Society, he has only to address a letter to the Secretary, who will immediately attend to his request.

These facilities, gentlemen, will, I hope, be an inducement for our amateur experimentalists to join the Electrical Society, by whose fostering care, and their own perseverance, they will gradually acquire dexterity and confidence in their performances, and will eventually become distinguished and valuable electricians.

There is another circumstance to which I must allude before I close my observations. It is intended by the Committee, that the surplus of the income derived from the subscriptions over the current expenditure, will be principally devoted to the publication of our transactions. This will give every member, whether present or not at our evening meetings, an opportunity of knowing the progress which the Society is making.

Before concluding this address, it perhaps may be necessary to allude to the circumstances from which this Society originated, as well as to the nature of the meetings which have hitherto been held under its auspices. In the spring of the present year, I delivered a course of lectures on electro-magnetism and magnetic electricity, at Mr. Clarke's, Philosophical Instrument Maker, Lowther Arcade. At the close of each evening's lecture, a conversation generally ensued among the gentlemen who honoured me with their attendance; and the want of a Society for the encouragement of electrical pursuits was occasionally spoken of, and universally acknowledged. On the 16th of May, a few of those gentlemen met; and after some discussion as to the best mode of establishing such a Society, it was agreed that the attempt should be made. The first of our meetings took place on the 10th June, and they were continued each succeeding Saturday, until the 12th of August, the number of members gradually increasing the whole of the time. On each evening, one or more papers were read, and several animated discussions took place, visitors being allowed, by the rules of the Society, to take a part.

Scarcely a meeting has passed without some new apparatus being produced on the tables; and I can, without hesitation, refer to the members, as well as to the visitors, for their approval of the order and regularity of our proceedings. The Society soon found it indispensable that they should have more accommodation; and the liberality of the council of the institution, under whose roof we are now assembled, has afforded us an opportunity of carrying out the original resolutions, even to a greater extent than we had anticipated.

In addition to the Theatre, the council have offered us the use of the apparatus belonging to the establishment; and I look forward, with confidence, that the close of the present session will find us sufficiently increased in number to enable the Committee to state in their report, that the Society is sufficiently matured to have its officers elected.

In conclusion, I may state that the Society has already secured the services of an able and efficient secretary, and of a Committee whose members are indefatigable in their exertions, and in whose hands the interests of the Society may, with confidence, be reposed, until its numbers are sufficiently augmented to form a permanent council and appoint its officers.

From the attention with which you have honoured me, gentlemen, whilst delivering this address, and the repeated demonstrations of approbation which you have so kindly manifested on the various topics which I have noticed, I cannot but flatter myself that, notwithstanding my inability to do justice to the subject I have ventured to discuss, I have succeeded in convincing you of the necessity for, as well as the prospects of, the London Electrical Society; and I have no doubt that as soon as the objects of this Society become more generally known, its utility will be proportionately appreciated; and its interests permanently secured, by the number and ability of its members, whose talents will soon be united in rendering it support.

October 14. Mr. Golding Bird read a paper on a new arrangement of the electro-magnetic apparatus. It would be difficult to describe the *modus operandi* of Mr. Bird's arrangement, without diagrams. The bars of the induced magnet are upright; over one of the poles of which is suspended a small ball of soft iron: on its being attracted, contact is broken. This destroys the magnetic influence; the ball regains its original position; and contact is renewed, which again induces powerful magnetism. It is thus, as it were, regulated by its own agency: and Mr. Bird considered that more powerful effects are thereby produced than by any other electro-magnetic apparatus.

In conclusion, Mr. Bird explained that he did not consider himself the inventor of a new apparatus, he merely pointed out new arrangements in the hopes of their becoming really useful.

October 21. Mr. Pollock read a paper on the change of form which the compound elements of a voltaic arrangement undergo; the transition of the zinc from the state of metal to that of oxide; the transition of the oxygen from the state it exists in water to that in oxide of zinc; and the change effected by the union of the acid of the solution with the oxide of zinc: the consequent increase and diminution of space occupied and the electric condition resulting. A paper was also presented to the Society, the result of actual experiments proving that voltaic electric currents can and do pass along a wire in contrary directions.

October 23. Mr. Bachhoffner read a paper which he illustrated by a number of experiments with the electro-magnetic coil. Mr. B. exhibited a battery weighing only 113 grains, which (with the asistance of a coil) would afford most intense shocks, as well as decompositions. In a former paper Mr. B. had stated that the insertion of a bundle of wires in the axle of the coil increased its power two fold—he would now say twenty fold. The chemical effects likely to be produced from the action of the coil offer a wide field of research.

November 4. Mr. Leithead. *Hon. Sec.*, read a paper on the circumstances attending the production of the various electrical phenomena. Mr. L. assuming that electricity is a fluid, proceeded to explain the varieties of electrical action by analogy. The existence of such a fluid not being known, all that is meant by the assumption is that the observed effect is similar to that which would take place if produced by a fluid subject to certain laws of action assigned to it, after careful experimental investigation. The mode of treatment was a comparison of the mechanical properties of elastic fluids, and the effects produced with those which are dependant upon electrical excitement; chiefly relating to the powers of attraction and repulsion.

Mr. Leithead considered that the presence of oxygen is essential to the development of electricity, both dry or ordinary, as it is termed, and voltaic; but in the latter it seems necessary that both hydrogen and oxygen should be present, for no voltaic action can be obtained without moisture: and it is not unworthy of notice, that while the passage of dry electricity between two bodies is always attended by the evolutions of light, the passage of voltaic electricity is usually accompanied by evolution of heat.

The author announced his intention of discussing the subject in a future paper, to be supported by experiments.

November 18. Mr. Pollock read a continuation of the paper presented by him on October 21, and replied to the objections then raised, which may be defined under three heads, viz.—1. Want of experiments to show the changes that take place. 2. Chemical action not the cause of all electrical action. 3. The phenomena may be explained by the examination of the properties of matter independently of any fluid. It is impossible in a short abstract to do justice to Mr. Pollock's paper. We believe it is the intention of the Committee to publish it in the transactions of the Society. But we may add that Mr. P. considers that although it might be possible to explain the phenomena of the battery according to the commonly received laws of matter, yet the balance of probability is in favour of the existence of a species of matter called an electric fluid, whose parts are so intensely small that the attractive force between themselves and surrounding matter exceeds that within themselves. This property is all that is required to explain the phenomena dependant on an electric fluid; and such matter must be highly elastic, highly diffusive, and strongly disposed to pass in the direction of least resistance.

December 5. Mr. Sturgeon read a paper on experimental and theoretical researches in electricity. This paper was the first of a series, and will appear in our next number.

December 16. A paper written by Mr. R. W. Fox, of Falmouth, was read, describing effects produced by voltaic action on clay. Several specimens of the clay were placed on the table, distinctly showing the laminated form in which that substance is arranged, when placed in an electrical circuit.

A paper was read by Mr. J. V. Moore, Assistant Secretary, translated by him from the one presented to the Acadèmie des Sciences of Paris, at its meeting of 30th Oct., 1837, and printed in their transactions.

This paper describes, very fully, the character and species of the insect which has obtained so much notoriety, in consequence of the experimental researches of Mr. Crosse. We understand, at the next meeting of the Society, a paper will be presented from Mr. Crosse, which we have no doubt will remove many erroneous impressions and place in a proper light the experiments and discoveries of a gentleman who has devoted so much of his time as well as his great resources to the science.

A paper was read by Mr. W. M. Higgins on the originality of Dr. Faraday's Voltameter, with an account of the instrument used by Mr. Robertson, as described by him in the Annales de Chemie of 1801.

IX. REVIEWS & NOTICES OF NEW BOOKS.

CHEMISTRY *as applied to the Fine Arts. By* GEORGE H. BACHHOFFNER, ESQ. *Lecturer on Chemistry.* CARPENTER AND CO., *Old Bond Street.*

A book of this kind has long been wanting amongst painters of every description, as well as amongst that superior class of them to whom the work before us is particularly addressed. There are several painters, however, who have a very good knowledge of the chemical properties of the materials they employ, both as regards their manufacture and their influences on each other. But it is a lamentable fact, that they who possess this highly essential qualification, are exceedingly few in number, when compared with the thousands who are entirely ignorant of any chemical process whatever. Man, however, is an imitative being; and progressively contributes to his previous stock of knowledge as new sources are presented to him, or as facilities are placed in his way, to the acquirement of that information he had previously wished to possess. This natural propensity, so strongly evinced in the wildest tribes of our race, is infinitely more manifested in civilized communities, where many and various motives for enterprise excite a spirit of emulation unknown to those nations who still remain uninitiated in the arts of social and domestic life. The laudable desire to "excel" is now so strikingly marked amongst our artisans of every description, that few are to be found, at the present day, who would like to be considered totally ignorant of the principles of their trade; or even incompetent of availing themselves of those advantages which a better knowledge of some particular point of their profession enables others to take. This, however, is the precise condition of those painters who are not acquainted with the chemical influences of the colours they employ, and how they are affected by the chemical agencies of the gaseous media to which they are constantly exposed.

Books on general chemistry are too large, and their most essential articles too diffuse, for the perusal of those artists who require no more chemical knowledge than that which is necessary to enable them to proceed, in a scientific manner, in the selection and composition of their colours. It is thus that Mr. Bachhoffner's "Chemistry as applied to the fine arts" will be found a valuable acquisition to those who have a desire to become scientific painters. It supplies the desideratum they have hitherto experienced; and places within their reach, and without the encumbrance of other matter, all the chemical information necessary for their profession. The author has made a good selection and arrangement of his subjects; and has given an extensive "table of the composition of the most important pigments" at the close of the volume. The work is very well got up, and is one which we would strongly recommend to the artist. We are glad to see a good list of subscribers' names attached to it; and have no doubt of its having an extensive sale.

ELECTRICITY. *Its nature, operation, and importance in the Phenomena of the Universe. By* WILLIAM LEITHEAD, ESQ., *Secretary to the London Electrical Society.* LONGMAN & CO, *Paternoster Row.*

The work before us is divided into two principal parts; the former of which may be considered as an introduction to the latter. The author has stated in his preface, that his performance "has no pretensions to the title of a scientific treatise; his object, in the *first* part of the work, being simply to explain the several varieties of electrical action as to enable the non-scientific reader to understand the *modus operandi* of the electric fluid, in producing the different phenomena which are treated of in the *second* part:" in which "he trusts that he has not advanced any proposition that is not founded upon, and supported by, acknowledged facts.

From our personal acquaintance with Mr. Leithead, and the respect we have always entertained for his abilities and candour, we are fully convinced that every part of his performance in this pleasingly written book is perfectly conscientious; and brought forward with the best of motives: that of being useful in furthering our knowledge in the important branch of science to which it is particularly devoted. We are, therefore, under no apprehensions of appearing unfriendly towards Mr. Leithead; by reminding our readers that there are too many authors who write on experimental science, who entirely lose sight of the important difference between " acknowledged facts" and *acknowledged assertions*: and we are sorry to observe in the work before us some excellent reasoning resting upon no other basis than the latter class of data. " Who," says our author, " has not at times experienced that painful depression of spirits, accompanied with unpleasant sensations of chillness, and other ' nervous' feelings, on one of those gloomy days without rain, and when the wind is easterly? The sensation of cold, at such times is attributable to some peculiar atmospheric influence, for there is no corresponding depression in the thermometer." This is certainly a very extraordinary fact; but we cannot " find a sufficient solution in the" *assertion* " that on such a day as we have described, the electroscope invariably indicates a negatively electrical state of the atmosphere:" because we are well convinced from our own experience (which is considerably extensive) that the electroscope indicates no such thing: nor do we know from what source the supposed "fact" has been derived. At page 279, Mr. Leithead has taken advantage of the *acknowledged assertion* that " electricity is said to move only along the *surface* of bodies;" and has ingeniously applied it to the explanation of what Majendie has said regarding the functions of the spinal marrow; viz. " it is at the surface of the organ that its properties, as far as regards motion and sensibility, are better unfolded."

Speaking of the last appearance of the *Spasmodic Cholera* in this country, Mr. Leithead says, " During the autumn, at the close of which this disease first made its appearance at Sunderland, thunder storms were more than usually frequent and violent. And, what is remarkable, a very short time before the irruption of the disease, that the nights were characterised by a highly electrical state of the atmosphere, and by the incessant discharges of the electric fluid in that form which has received the appropriate term of *silent lightning*, owing to its being unaccompanied with thunder."

We do not remember meeting with this " appropriate term" before; nor do we think that it expresses any thing more definite than *silent thunder* would do. For it is on no other account than that the storm is too distant, that the thunder accompanying this lightning is not heard by the observer. "Sheet lightning" is the usual term for such appearances, which, if to " windward" never deceives the experienced observer, of the speedy approach of the electric storm.

If the electric state of the atmosphere exhibited any peculiarities at the time mentioned in the preceding extract, the circumstance, connected with the appearance of the *cholera*, is very interesting; and Mr. Leithead has turned the connexion to good account. But we are afraid that his argument is not much strengthened by the following data. "During the whole period of the prevalence of the malignant cholera at Liverpool, in the fall of the year 1832, the magnetic needle (adapted with the collecting wires so as to be deflected east or west, accordingly as the atmosphere was positive or negative) continued steadily to point about eleven degrees west of north, thus indicating a constantly negative state of the air, &c." As the object of our remarks is not to find fault with observers, but, if possible, prevent future error, we will merely say, that we have seen the instrument by which these observations were made, and are perfectly convinced that neither the motions nor positions of its needle are indicative of the electrical state of the atmosphere. We have no room at present for further remarks on Mr. Leithead's "Electricity." But, because of the important matter contained in the second part of the work, its judicious arrangement, and the interesting inferences which accompany it, we mean to read it over again, and may possibly be led to offer some further observations to our readers. In the mean time we recommend it to the attention of the physiologist and metaphysician, who will discover a considerable display of talent, without ostentation, in their respective departments of research.

X. MISCELLANEOUS ARTICLES.

We have just received a letter from Dr. Hare, Professor of Chemistry, in the University of Pennsylvania, from which the following is an extract.

"I have just obtained a new nitrous ether which is sweet to the taste. boils at 60° F. affecting the palate with the savoury taste and after gout of acridity which distinguishes cinnamon. It smells like chlorine ether, obtained by mingling chlorine with olifant gas."

I am, yours truly,
ROBERT HARE.

Mr. Wm. Sturgeon.

Rejoinder to Mr. R. W. Fox's note in the first volume of the Annals of Electricity, page 507. By W. J. Henwood, *F. G. S., Secretary of the Royal Geological Society of Cornwall, H. M. Assay Master of Tin.*

To William Sturgeon, Esq., *&c. &c. &c.*

My dear Sir,

Mr. R. W. Fox has neither fairly met nor answered the objections which I have made (Annals of Electricity, I, 228,) to his explanation of his modification of M. Becquerel's experiments. My own experiments, although more simple than those of M. Becquerel, are like them, but unimportant variations from the distinguished Frenchman's mode, and their *results*, where comparable, are similar. We differ only as to the *explanation* of them. Mr. R. W. Fox thinking the appearances presented, to result from the decomposition of the *solid* (the pyrites, or double sulphuret of copper and iron) by the abstraction of the iron and a part of the sulphur; whilst I (following M. Becquerel) believe them to be in consequence of the decomposition of the *sulphate of copper in solution*, and the *deposit* of the *sulphuret of copper on the pyrites.*

If the ore "has been changed to some depth," the abstraction of the sulphur and iron would have shown a diminution of its weight; without its being "stripped" in the manner described (Annals I, 507).

Indeed, it is obvious that this process (so superfluous if his explanation be the true one) cannot be practised without some of the pyrites being also "stripped" off.

I think it is plain that instead of meeting my objections by *experimental* proofs, it is intended to get rid of them by a "side wind."

I remain, my dear sir,
Yours very faithfully and respectfully,
W. J. HENWOOD.

4, *Clarance Street, Penzance,*
October 30, 1837.

P. S. I have already (Annals I, 132,) expressed my dissent from the theory of the origin of metalliferous veins, which Mr. R. W. Fox has recently propounded; and I shall be ready, whenever an *unobstructed* opportunity may occur, or it may be required, to produce evidence derived from the mines of Cornwall, which I think will be conclusive against it.

From the number of orders for electro-magnetic coil machines, and the enquiries respecting the particulars of their structure and price, which have been received since the publication of the last number of the Annals, we are led to believe that the postscript at page 484 has conveyed a very different idea to that which was intended. We therefore beg permission to say that we cannot possibly attend to any more orders for these instruments. They are now pretty well known to the London Instrument makers, to whom we must refer those gentlemen who are still in want of them. EDIT.

In answer to our correspondent who wants to know "if it be possible to suspend a needle in the air by transmitting an electric current through a helix in which the needle or bar is lying," we must say, yes. The fact was first shown at the London Institution, Moorfields. The battery employed was contrived by Mr. Pepys, and consisted of a single pair of plates, of copper and zinc, each about 50 feet long, and 2 feet broad; formed into a spiral on a cylindrical nucleus of wood, and placed in a barrel or circular wooded trough, which held about 50 gallons of acid solution. The experiment may be made, however, with a battery of one square foot of each metal, immersed in a strong solution of nitrous acid. The helix must be of narrow bore; of 6 or 8 layers of spirals, and held vertically. The gravitating propensity of the needle may be much reduced by holding a bar magnet at a small distance above the helix. EDIT.

We have received from our correspondent at Munich, Gauss and Weber's "Resultate aus den Beobactungen des Magnetischen Vereins im jahre 1836." Also the translated part of that valuable work; which shall soon have a place in the "Annals of Electricity, &c."

As we are confident that the English reader will be highly interested with the progress of science in Germany, we intend, whenever we have room in these Annals, to publish all the novel matter in experimental science that we can procure from that country. Our readers may also expect much information from France, Italy, and America. EDIT.

THE ANNALS
OF
ELECTRICITY,
MAGNETISM, & CHEMISTRY;
AND
Guardian of Experimental Science.

CONDUCTED BY

WILLIAM STURGEON,

Lecturer on Experimental Philosophy, at the Honourable East India Company's
Military Seminary, Addiscombe, &c. &c.

AND ASSISTED BY GENTLEMEN EMINENT IN THESE DEPARTMENTS
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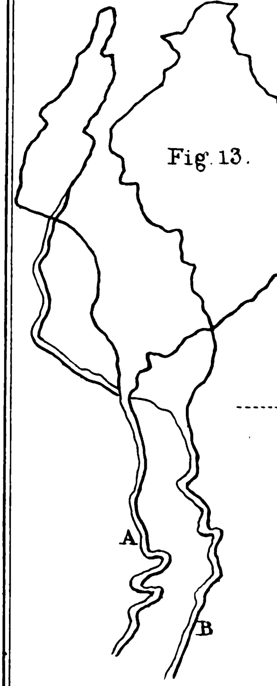

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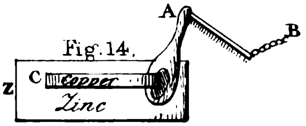

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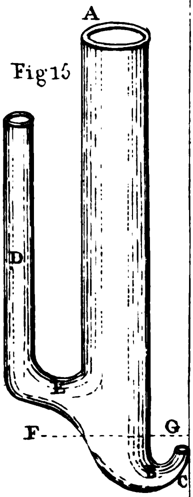

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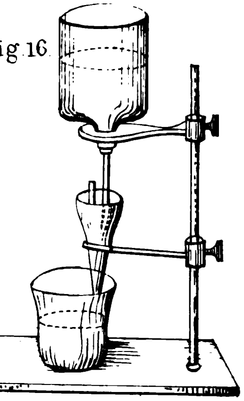

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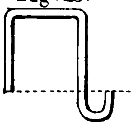

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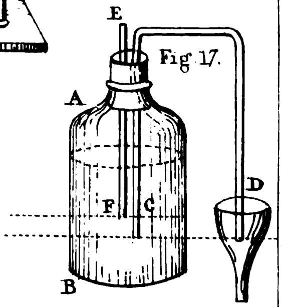

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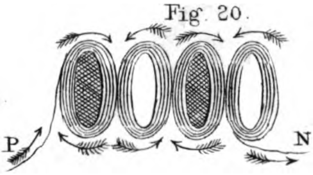

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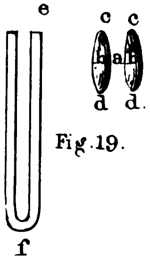

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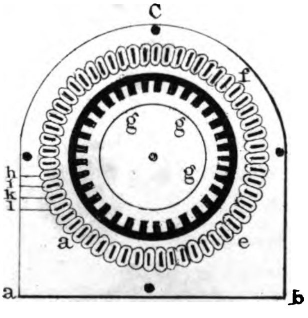

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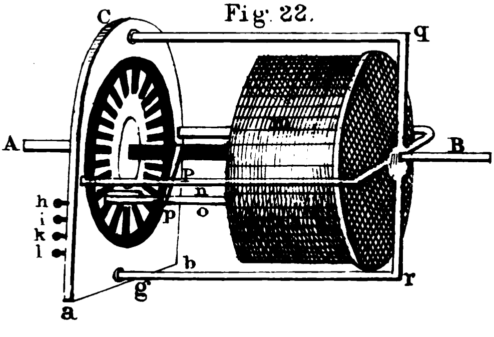
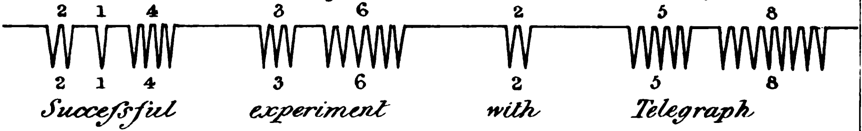


W. Annan lith. 12. Gracechurch St.

Fig. 23.

Specimen of Morse's Electro Telegraphic Writing



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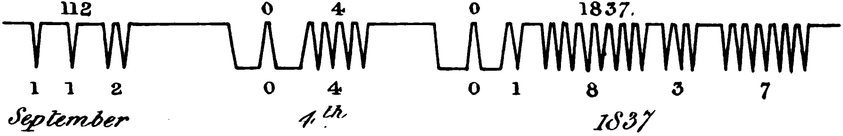

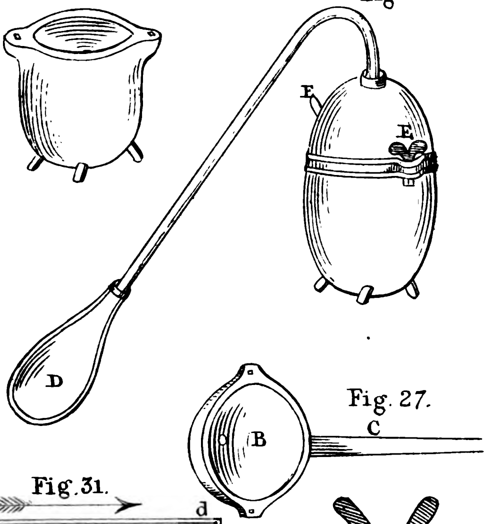

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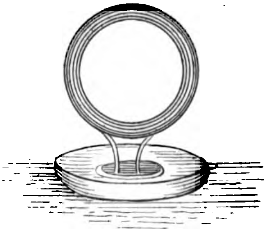

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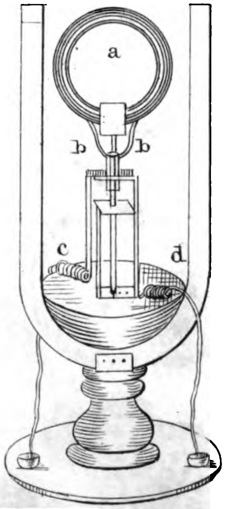

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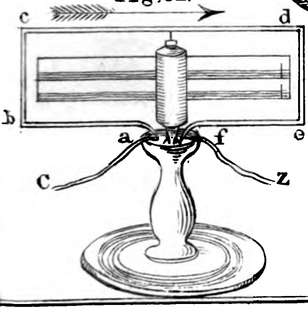

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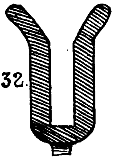

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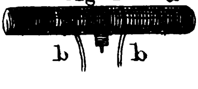

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THE ANNALS
OF
ELECTRICITY, MAGNETISM,
AND CHEMISTRY;
AND
Guardian of Experimental Science.

FEBRUARY, 1838.

XI. *On the protection of Ships from Lightning.* *By* W. Snow Harris, Esq. F. R. S. &c.

To the Editor of the Annals of Electricity, Magnetism, &c.

Dear Sir,

The best reply to Mr. Roberts's remarks on my method of defending ships from lightning, and which appeared in your last number, is an appeal to facts. I may hence observe, that my system has been partially adopted in the British Navy in about eleven ships for as many years, comprising several line of battle ships and large frigates. These vessels have all been more or less exposed to lightning, and cases have occurred in which the electric discharge has fallen heavily upon the masts without any ill consequence; nor have any results of a mechanical kind attended the application of my conductors, detrimental to the masts; on the contrary, the spars are greatly improved in strength by them. I extract, for the information of your readers, from the Nautical Magazine, the last report from the Beagle, lately returned from a five years' hazardous survey in the South Seas. This ship has again proceeded on another similar voyage with the same spars and conductors fitted in them:

"Report on the lightning conductors of H. M. S. Beagle, 1831—6. Previous to sailing from England in 1831, the Beagle was fitted with the permanent lightning conductors invented by Mr. W. Snow Harris, F. R. S.

"During the five years occupied in her voyage, she was frequently exposed to lightning, but never received the slightest damage, although supposed to have been struck on at least two occasions.

"At each of these times, at the instant of a vivid flash of lightning, accompanied by a crashing peel of thunder, a hissing sound was heard distinctly on the masts, and a strange, though very slightly tremulous, motion in the ship herself, indicated that something unusual had happened.

"No objection, which appeared to me valid, was ever raised against them; and were I allowed to choose between having masts so fitted, and the contrary, I should decide in favour of those with Mr. Harris's conductors. Even in such small spars as the Beagle's royal masts and flying jib-boom, the plates of copper held their places firmly, and increased rather than diminished their strength.

"The Beagle's masts, so fitted, answered well during the five years' voyage above mentioned, and are now fit to go on another equally long voyage."

(Signed) ROBERT FITZ ROY,
Late Captain of H. M. S. Beagle.

As one fact is worth a thousand theories or loose opinions, I should suppose this report must go far to meet Mr. Roberts's assertion; viz., that "if my conductors were thick enough to be efficient, they would injure an essential quality of the mast."

I do not by any means wish to set limits to freedom of discussion in scientific matters: science almost invariably gains by collision of opinion; but I may be here allowed to observe, that before any individual ventures to depreciate the invention of another, with the manifest object of a more complete establishment of his own, it is certainly his duty to make himself very fully acquainted with the subject. Mr. Roberts has not thought this, however, worth while; or, I am sure, he would not otherwise have so loosely adverted to my system, or attributed to me opinions which I have never professed to hold. I have nowhere maintained, for example, as my opinion, that "superficies, *not content*, conducts electricity;" or have I, upon the *validity* of such a principle, let into the masts "*strips* of copper of *little* thickness." If your readers will be so good as to refer to my papers in the late numbers of the Nautical Magazine, New Series, Vol. I, Nos. 11 and 12; or Old Series, Vol. III, Nos. 33 and 34: they will find my views on this point very sufficiently detailed;* and will see I have duly considered the mass of my conductors. Their thickness is about the same as that of the present chain conductors employed in the Navy, and they contain *twenty* times as much metal. Their average value is that of an iron rod of more than two inches diameter, supposed to extend from the truck to the keelson of a frigate of 50 guns. Mr. Roberts does not inform us of the diameter of his wire rope; I should imagine it could not be considerable, in consequence of its great weight; he merely says, "let some hundreds of fine annealed copper wires be laid up, as a common hemp rope, &c.," an indefinite sort of expression, which may either apply to the cables or the signal halliards.

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* See sections 65 and 73.

Mr. Roberts also greatly mistakes the nature of my plan, when he says there "must be a separation" to allow of the mast being lowered; that is, if he means by this form of expression, a disjointing of any consequence to the action of the conductor. The contact, as may be seen by reference to my drawings, may at all times be insured, and that too, without any difficulty. I do not myself believe even if there did occur a short interval in the caps, that it would be of the least consequence to the action of such an extended and continuous line of metal armed with a point such as I employ, and which is always most perfectly continuous below the mast head. Such a break, however, as that inferred by Mr. Roberts's observation, need not occur at all; it is scarcely worth while therefore to discuss the question.

I deem it requisite also to state, that if I have ever given an instance of serious injury "arising to a sailor leaning against a mast," it must have been a mast *not fitted* with the conductor. In such an instance it is evident the man's body became for a short distance the *conductor* to the mast; hence the electrical discharge led through the former, so far as it went.

I do not myself believe in what Mr. Roberts calls a lateral explosion; if by that, he means a divergence of the electricity actually transmitting by the conductor, from its determinate course. It has certainly no existence during the passage of heavy electrical discharges through metallic conductors of large electrical capacity. Any one may satisfy himself of this by discharging a powerful battery through a copper wire of about one eighth of an inch in diameter, or a small brass tube, held in the hand. I have myself repeatedly discharged batteries of 40 square feet of coated glass highly charged, in this way, without experiencing any sensible effect whatever.

I do not pretend to deny but that in every case of an atmospheric electrical discharge, there is a general induction upon the *whole* mass of the *vessel*, as forming one of the great electrified surfaces. The distribution of its electricity, therefore, *previously* to the discharge, becomes changed; and will in all cases be again restored as the forces in operation become neutralized. Mr. Roberts should not be ignorant that this effect can in no way be got rid of, and that it cannot be fairly urged as an objection to my conductors. On the contrary, my system, not being confined to a mere wire line in the rigging, provides for an easy and rapid neutralization of the opposite electrical forces throughout the hull; a matter of very considerable importance.

Admitting the objection, however, of a lateral explosion to be a valid one, it necessarily applies equally, if not more forcibly, to Mr. Roberts's rope of wire, than to my plates of copper, as I think must be apparent; since the wire is directed to be laid along the back of the mast and stopped to the rigging; of course it must be liable to contact with the sails. What material I would ask is more likely to catch fire than a tarred rope or a sheet of canvass; we find this in numerous cases of damage by lightning at sea; as, for instance, in the cases of the Thisbe, Phaeton, Southampton, and other ships of the British navy.* It is, in fact, the liability to this kind of damage, though not from any lateral explosion, which renders the application of conductors of *small* electrical capacity to the rigging and masts, such as those commonly employed, somewhat precarious.

The objection to my conductors on the ground of their being near the magazines, is certainly one I should not have anticipated, seeing that lightning rods are applied, either immediately, or otherwise within a few feet of almost every powder magazine in Europe. It is, in fact, because the masts (which are already, be it remarked, conductors of electricity to some extent) pass in that direction, that it actually becomes necessary to protect them by a conductor connected with the sea. For, as remarked by a practical writer on those subjects in the Philosophical Transactions, the danger is over when the electric fluid has reached the well.

It is further a well-known electrical principle, that lightning will not leave a good and efficient conductor, *immediately in the line of its action,* to pass upon bad conductors, *out of that line:* a principle which quite vitiates an opinion advanced by Mr. Roberts, that every joint in a chain conductor becomes a point "where the electrical fluid may strike off in every direction." But the electric discharge is never found to *strike off* from the chain, if connected with the sea or ground. It invariably pursues its course down it, as may be seen by reference to the cases of her majesty's ships, Ætna, the New York Packet, and the Plymouth Church.* The danger here, is in the conductor being fused and disjointed at these points, and the temperature so raised as to set the rigging on fire. This would be very probable if the chain or rope employed as a conductor became detached from its connexion with the sea, or broken high up. It was from this cause that a discharge through a chain on board the Lion, of 64 guns, knocked down one of the quarter-masters on deck; and in a similar way a stroke of lightning came down the chain top-sail sheets of the Ville de Lyon, a large American packet, which lately put in here to repair the damage which ensued, and killed two men. We see, therefore, that since a conducting chain or rope is liable to every species of damage incidental to a ship's rigging, it may at the time of handling it, produce fatal consequences to the seamen; it being then not a conductor in a free state, but approximating to an insulated charged conductor, ready to strike off to the next conducting substance.

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* Nautical Magazine, in the Nos. above mentioned.

It is not with any unfriendly feeling that I am led to express my surprise at the objections urged against my system by a gentleman who says he has turned "his attention to the subject, as well in a philosophical as in a nautical point of view," and who, whilst professing "by an examination of the causes, and a citation of a few effects, to solve the embarrassment under which we labour," fails in the course of his paper to produce any kind of "examination of the causes" whatever, any *original* enquiry in electricity, or any one accredited instance of damage by lightning, but substitutes for the essential ingredients of observation and experiment, either objections which have not even novelty to recommend them, or otherwise notions of ordinary electrical actions, not warranted by any known fact. It is even problematical whether the wire proposed by Mr. Roberts is available, in the way he recommends; at least it comes before us in so questionable a shape, that some sort of experience of it seems requisite to establish its value.

I must shorten this long communication by referring your readers to my papers in the Nautical Magazine; in which they will find my system very fully explained: they will then see that it is of a far different kind and tendency to that which might be inferred from Mr. Roberts's remarks. I shall merely observe, therefore, that it consists, 1st. in perfecting the conducting power of the masts themselves, by giving them conductors of great electrical capacity. 2d. In tying these conductors and the detached masses of metal in the hull, into one general system, and finally connecting the whole by efficient conductors with the sea.

The advantages of my system are these.—The conductor on the masts is always in place, and hence ready to meet the most unexpected danger; it does not require, like a chain or rope, a constant watching and attention, to the great annoyance of the seamen, but takes care of itself. The standing or running rigging is not in any way interfered with, and a very perfect continuity is arrived at under all the varying positions of the mast. It is permanently fixed throughout its whole extent, gives stability to the mast, is continuous from the sea to the mast-head, and is connected with an adequate combination of conductors in the hull to satisfy the most powerful discharge of lightning yet experienced; it is capable of resisting great external force, and in case of the removal of any portion of the mast, either by accident or design, the remaining portion is always perfect and adequate to the required protection. It has further the capital advantage of being applied immediately to the objects most requiring it, viz: to the masts themselves, by which the conducting power they possess is turned to a beneficial account.*

Your readers will, I trust, perceive, that in replying to Mr. Roberts's paper, I have studiously adhered to facts, either depending on actual *observation* on the great scale of nature, or otherwise deducible from *experiment*; and they will, I hope, further do me the justice to believe that I profess to hold no opinion which can be *fairly* shown to be inconsistent with these two great oracles of physical science.

I am,
Dear sir,
Yours &c.,
WILLIAM SNOW HARRIS.

XII. *An Investigation on the cause of the Fracture of Jars during an Electric Discharge; and on the mode of protecting them. By* William Sturgeon, *Lecturer at the Honourable the East India Company's Military Academy, Addiscombe, &c. &c.*†

Perhaps, no circumstance whatever tends more to damp the spirit of philosophical enquiry, or to retard the progress of scientific pursuits than that of expensive apparatus, and the liability of spoiling various parts of it by the process of experiment, without ever attaining any thing like a satisfactory result. Experiments are frequently attempted, both in chemistry and electricity, which, if unattended by misfortune, might lead to the most important results; but the breaking of a retort, or a jar, in one moment defeats the object, and the experimenter, by this unfortunate circumstance, abandons, perhaps, altogether, the subject he was ardently pursuing. Any attempt then, however humble, that can possibly aleviate the embarrassment of the experimenter, under such painful and discouraging circumstances, must necessarily be considered of some importance: not only in any particular enquiry, but to the encouragement of scientific pursuits in general.

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* Let a small iron wire be taken and the quantity of electricity measured just necessary to fuse it. Insert a similar wire from the same reel; along one side of a small cylinder of wood in its ordinary state, and discharge upon this wire the same quantity of electricity. The wire will now remain perfect, being assisted by the wood, and will not become fused except by the addition of a very much greater quantity.

† Read before the London Electrical Society, on Saturday, January 6th; and published by permission of the Committee.

Electric jars, when charged intensely, it is well known, are frequently perforated, or starred, as some persons call it, on being discharged. Many experimenters break their jars by the unscientific practice of placing one of the balls of the discharging-rod against the side on the coating, whilst the other ball is made to approximate that of the jar, and which communicates with the lining. For, with this disposition of the discharging-rod, it is evident that, when the discharge takes place, the whole force of the concentrated fluid, which before was disseminated over the whole area of the lining, is now suddenly impressed on that particular point of the coating immediately in contact with the lower ball of the discharging-rod: and the glass, not being sufficiently strong to sustain the shock, is frequently perforated; and then (as to electrical purposes) rendered entirely useless.

Misfortunes of this nature might easily be averted, by placing the jar on any good conducting substance, such as a piece of tin foil, or on a plate of any other metal: for, by placing the lower ball of the discharging-rod on the metallic plate, the fluid will be dispersed over every part of the bottom of the jar at nearly the same moment, and thence equably disseminated over the rest of the coating without the possibility of breaking the jar by the whole force acting against any particular point. This precaution, however, although necessary to be observed whenever a jar or battery is discharged, is by no means a complete protection: for jars are frequently broken although every possible attention be paid with respect to the application of the discharging-rod: and it is well known by every person familiar with experimental electricity, that when a large battery is discharged, one jar, at least, is almost sure to be broken: and so little has the *cause* of this phenomenon been understood, that no method is yet before the public, with which I am acquainted, to remedy the expensive and highly discouraging evil. It is, therefore, a common practice with electricians, not to charge their batteries to a high intensity, lest some of the jars should be broken by the discharge.

The only direction given by authors on this subject, for preserving intensely charged jars, is that of transmitting the discharge through a long circuit. But even by this precaution, the end is not answered; for, by transmitting the discharge through a long circuit, the effect is considerably reduced: and, therefore, nothing more is obtained by this means, than by transmitting a charge of lower intensity through a short circuit.

The frequent misfortune of breaking jars by the discharge, and the consequent expense and trouble attending the fitting up of new ones, led me to the determination of investigating the cause, and, if possible, to remedy the evil; and I am happy to say, that this investigation has enabled me to accomplish the object to the utmost of my wishes.

Before entering on a description of the method I have taken to prevent jars from breaking by the electric discharge, it may, perhaps, be necessary to premise by pointing out such particulars as were noticed during the enquiry, and which seemed most worthy of attention.

The first jars used in these experiments were fitted up in the usual way, by coating and lining them to within about three inches of the top. The wire of each jar, surmounted by a ball, had its lower extremity passed through the cover, or lid, and looped to a chain, the lower end of which rested on the bottom of the lining.

When jars fitted up in this manner broke by the discharge, the fracture, or star, always occurred at nearly the top of the lining. Now, as the joining of the chain with the lower extremity of the wire was nearly opposite to the point where the fluid perforated the jar, I concluded that the chain, in consequence of the interruptions at the links, was incapable of conducting the fluid from the bottom of the jar: and that, instead of the whole quantity being transmitted from the bottom to the top of the chain, the greatest portion of the collected fluid escaped by one sudden effort from the upper edge of the lining to the lower extremity of the wire: and thus, nearly the whole force of the fluid being discharged by explosion at one particular point of the lining, the reaction against the side of the jar caused the latter to be broken, for the same reason that jars frequently share the same fate by having the lower ball of the discharging-rod placed against their sides.

Jars were afterwards fitted up without any chain, having one continued wire from the ball on the top, to the bottom of the jar, in hopes that if the wire was sufficiently stout, the whole of the collected fluid would be transmitted in safety throughout its whole length. Hence the explosive effort from the lining to the wire being thus supposed to be annihilated, the jar would be preserved. It was not long, however, before I discovered my mistake; for a jar, so fitted up, had not been charged and discharged more than five or six times before it was perforated by the fluid close to the upper edge of the foil. Some others were fitted up in the same manner, only using stouter wire from the ball to the bottom of the lining: still impressed with the idea, that if the wire was sufficiently capacious, the fluid would be safely conducted to the discharging-rod without injury to the jar. Some of these jars stood the discharge from high intensities extremely well, and, perhaps, much longer than if the wire had not been so stout: nevertheless, as some were perforated in exactly the same manner as those before described, it was evident that this mode of fitting them up was no real protection; and that some other must be resorted to before intense charges could be transmitted through short circuits with safety to the jars.

The jars used in these experiments were of green glass, and broke most frequently when several of them were used at the same time in the form of a battery. I generally used six or eight at a time. I have, however, had the misfortune to fracture several of white flint glass: one of which exposed a coated surface of nearly three square feet. This jar stood very well at moderate charges, but was perforated by the first discharge of intense electrization. It would be needless to enumerate more of these disastrous circumstances, as they occur frequently, and in the hands of every electrician.

Now, as so many jars had been broken exactly opposite the upper edge of the lining, it would seem as if the collected fluid made the greatest effort to escape from that particular part: and, if it be admitted that the particles of the electric fluid are repulsive of each other, it is only reasonable to suppose that such would be the case: for, although an equable dissemination may actually take place over every other part of the lining, yet the naked part of the jar above the lining not being charged, the fluid about the edge of the foil, finding a less resistance upwards than in any other direction, would accumulate to the greatest extent around the upper edge of the lining: and, consequently, would have a greater tendency to strike the wire from some point near the top of the lining than from any other part of the jar.

Although there are many jars that will withstand the utmost force of a discharge, nevertheless, there can be no doubt but in every jar that is fitted up in the usual way, the fluid strikes the wire from the upper part of the lining in almost every discharge from high intensity: and that those jars, which have not broken, owe their safety to the strength of the glass. This conclusion may, I think, be fairly admitted when it is considered that many jars will withstand the shock for hundreds of times although one of the balls of the discharging-rod be placed against the coating at every discharge: whilst many others, which are of more feeble glass, have broken at the first discharge, by this unskilful application of the discharging-rod.

If due attention be paid to the nature of the electric jar, and the manner in which it is usually constructed, it will appear evident that, during a discharge, if the whole quantity of fluid from the lining were to flow through the conducting channel purposely arranged for it, that portion which occupied the upper part of the lining would have to travel by a very circuitous, and, perhaps, injudiciously chosen route. It would first have to descend to the bottom of the jar before it arrived at the axial wire; thence the whole length of that wire before it arrived at the ball on its top. Moreover, whilst ascending the axial wire, the fluid would have to pass the very point from whence it first set out. Now the electric fluid has never been understood to evince any tendency to move by a circuitous route, and more particularly so when one portion of it has to *meet* or *pass close to*, another portion, in a *recurved* circuit, similar to that presented by the lining and axial wire of a jar. Moreover, it appears to me, that the greater part of the axial wire, which is *within* the jar, is in a *negative* condition, relatively to the lining and upper part of the stem and ball, from the moment the jar ceases to receive fluid, till it is discharged; and this negation would be increased, for a moment, by the presentation of the discharging rod to the ball of the jar: and thus place the lower part of the axial wire in very ample condition for the reception of the fluid from the most vicinal and intensely charged point of the lining; which, as already assumed, would be in some part of its upper edge. There can be no just reason to doubt, however, that some part of the charge actually makes its escape by the chain from the bottom of the jar: but the proportion of fluid discharged by this route is probably very small: and, especially in very tall jars, such as are most frequently selected for electrical purposes; where the fluid, in some cases, would have to traverse two or three feet of lining, chain, and wire, and yet be no nearer the ball on the top of the jar, than it was before the approach of the discharging-rod.

Now, since the conclusions drawn from this investigation show that the electric fluid in the interior of an intensely charged jar indicates the greatest tendency to escape from the top of the lining; and that by the present mode of fitting up jars, an explosion from that part of the lining to the wire probably takes place whenever the jar is discharged through good conducting media, it seemed the most natural method, to *lead* the fluid, as it were, by some good conducting substance by the nearest route, and dispense altogether with the wire and chain that are suspended in the axis of the jar. For this purpose, two slips of tin-foil were secured to the opposite sides of the jar, and reached from the upper edge of the lining to the cover on the top; the under part of which was also covered with foil. This latter portion of foil communicated with the lower extremity of the wire which supports the ball: so that a complete and direct metallic connexion now existed between the top of the lining and the ball on the top of the jar. Therefore, no explosion in the interior of the jar could possibly take place. The result was, that every jar so fitted up has hitherto withstood the most severe trial. I have, for the last twelve years, employed jars thus protected, without ever breaking one by a discharge: although, during that period, I have discharged a battery of twelve jars some hundreds of times from the most intense electrization. I have called the slips of foil *protectors*: and I am firmly persuaded, that the most extensive battery may, by this means, be perfectly protected.

The next question that naturally presents itself is, does the insulation continue as perfect in jars thus protected, as in those fitted up in the usual way?

Upon attentive examination, it will be found, that the ball and wire of the ordinary jar, must always be electrized equally with the lining with which they are connected by means of the chain; and therefore the insulation from the lining to the coating, in such jars, can only be from the centre of the cover through which the wire passes to the upper edge of the coating: and as the cover is of wood, which is always a partial conductor, it also becomes charged in common with the wire and lining. Therefore, the only perfect insulation is that between the edge of the cover and the top of the coating—exactly the same as in jars furnished with protectors.

When jars are cylindrical throughout, no covers need be used. A disc of wood of nearly the same diameter as the interior of the jar, is fixed by wedges of cork at the same height as the top of the lining. This disc is covered with tin foil, and the lower end of the wire carrying the ball is screwed into its centre. In addition to the cork wedges, I usually support the disc by three wooden rods, which rest on the bottom of the jar.

It is not my intention, however, to press the theoretical part of this paper too strongly on the attention of the society; because I am well aware that unless experiments were made to show that the fluid actually leaps from the upper edge of the lining to the axial wire of ordinarily fitted up jars, it might lead to unnecessary doubts in the minds of those who have paid but little attention to the pursuit of a cause whose effects are the most disheartening that the amateur electrician has to contend with.

I have pointed out what has appeared to me to be the cause of these accidents to jars; and have briefly described the mode of investigation, both mental and experimental, which I pursued: and, whether my theoretical views be considered satisfactory or otherwise, the simple fact, alone, of my not having broken even one jar, thus fitted up, although I have constantly employed them for the last twelve or more years, during which, but few have had more extensive practice, may, perhaps, be sufficiently important to induce other electricians to adopt the same mode of protecting *their* jars which, for so long a course of practice, has afforded a complete protection to mine. If I have succeeded in this particular, the principal object for offering this paper to the notice of the Electrical Society will then be accomplished.

Westmorland Cottage,
Jan. 3, 1838.

XIII. *Memoir on Volta's pile and on the general law of the intensity of currents, whether arising from a single element or from a pile of great or small tension.* By M. POUILLET.*

1. The intensity of the electric current caused by any of Volta's piles, may be measured by the chemical, physiological, or physical effects which this current is capable of producing. In the course of this memoir, we have taken as the unity of measurement the physical effect that this current exercises on a magnetic needle; because this effect has the advantage over all others of being produced instantaneously and being measurable with the greatest degree of accuracy. But the results given by this unity of measure are not without con-

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* Translated by Mr. J. H. Lang; from the Comptes Rendus, hebdomadaires des séances de l'Académie Royale des Sciences, No. 8, 20th Février, 1837.

nexion with those which would be obtained by taking for unity either the chemical or physiological effects: on the contrary, there always exists such a dependance between them, that the first may be deduced from the second, and *vice versâ*. This connexion between effects apparently so different, and sometimes so completely opposite, is an important point in the theory of the pile: it explains what has been hitherto called the tension of the electricity in the current, and also how it is that a pile which is very energetic in producing physical effects, may be very feeble in producing chemical or physiological effects, and reciprocally. However, until these explanations can be deduced from experiments, it is well to recollect that the intensities which are here compared, are only electrodynamic intensities, or those of actions exercised on a magnetic needle placed under similar circumstances.

2. *Description of the apparatus.* The piles which we preferred employing, were *elissonated* piles; for the principle of which we are indebted to M. Becquerel (*Ann. de Chimies*, t. 41, p, 20.); they possess the immense advantage of keeping a constant force for several hours, and, consequently, giving perfectly comparable results.

The intensities of the currents were measured by two different apparatus: one called the *compass of tangents;* the other the *compass of sines.*

The compass of tangents is composed of a large ribbon of red copper 1^m, 6. long, 0^m,02. wide, and 0^m, 002. thick. It is covered with silk and bent so as to form a very exact circle of 0^m, 412, in diameter: the two extremities of the ribbon are brought together and prolonged on the outside so as to immerse each in a vessel of mercury, where they receive the current. This circle is placed vertically, and from its centre is suspended, by a silk film, a magnetic needle 5 or 6 centimetres long, bearing a light blade of wood or metal, 16 centimetres long: this blade serves as an index, since its extremities move on the circumference of a divided circle. The ribbon circle being in the magnetic meridian, the magnetized needle is at zero, and as soon as any current passes this circle the needle is driven towards the east or west to a certain extent, which depends upon the force of the current. When the equilibrium is established, that is to say, when the effort of the terrestrial magnetism to keep the needle in the meridian is equal to the opposite effort which the current makes to drive it from it, the intensity of the current is measured by the *tangent* of the deviation of the needle.

The compass of sines is composed of a strip of red copper analogous to the preceding one, but bent in the form of a rectangle; the large horizontal sides being 2 decimetres and the small vertical ones, from 5 to 8 centimetres, according to the degree of sensibility required; this rectangle is placed on a graduated circle of which it forms in some manner the cross-staff, and a magnetized needle is suspended in the rectangle, so that its centre may be in the vertical plane of the centre of the circle. When a current passes through the rectangle, the needle varies; but we follow its motions so that it be always in the vertical plane of the rectangle when it stops, maintaining an equilibrium between the terrestrial force and that of the current. In this case the intensity of the current is in proportion to the *sine* of the deviation of the needle.

For very feeble currents, the two preceding compasses have a multiplier instead of a simple circuit.

3. *Experiments with a single element.* To determine the different degrees of diminution, which the intensity of the current produced by a single element, undergoes, when it is made to traverse circuits of different lengths, we take pieces of wire of copper, platina, silver, iron, &c., covered with silk, proceeding each from a similar piece of metal, drawn out with such care that the diameter of the wire may be sensibly the same throughout the whole of its length: from each piece we cut five smaller ones of different lengths, for example, 5^m, 10^m, 40, 70, and 100 mètres for wires of about a millimètre in diameter; and 0^m2; 0^m4; 1^m; 2^m; 4^m; and 10^m for wires of small diameter.

With each series of wires we made the following experiments:—

The current produced by the voltaic element subjected to experiment was made to traverse the compass directly, and the deviation was observed; we afterwards made the current traverse successively each of the five wires, carefully observing the corresponding deviation.

The following table contains the result of one experiment.

Length added to the primitive circuit or to the length of the element.	Deviation observed.	Tangent of the deviation or intensity of the current.
0^m	64° 30'	2·100
1	35 15	0·707
2	24 00	0·445
4	13 40	0·243
8	7 30	0·132
16	3 40	0 064

At first sight, there seems to be no regularity in the decrease of the intensity; but we must not consider only the wire added to the primitive circuit but we must also take account of the primitive circuit itself; representing its unknown length by x and admitting the intensities of the current to be in a proportion inverse to the total length of the circuit, we form the five equations.

$$\left.\begin{array}{l} \frac{x}{x+1}=\frac{707}{2100} \\[4pt] \frac{x}{x+2}=\frac{445}{2100} \\[4pt] \frac{x}{x+4}=\frac{243}{2100} \\[4pt] \frac{x}{x+8}=\frac{132}{2100} \\[4pt] \frac{x}{x+16}=\frac{64}{2100} \end{array}\right\} \begin{array}{c} \cdot\ \ \cdot\ \ \cdot\ \ \cdot \\[4pt] \cdot\ \ \cdot\ \ \cdot\ \ \cdot \\[4pt] \text{whence we have}\ \ \cdot\ \ \cdot \\[4pt] \cdot\ \ \cdot\ \ \cdot \\[4pt] \cdot\ \ \cdot\ \ \cdot \end{array} \left\{\begin{array}{l} x=0{\cdot}51 \\[4pt] x=0{\cdot}54 \\[4pt] x=0{\cdot}52 \\[4pt] x=0{\cdot}53 \\[4pt] x=0{\cdot}50 \end{array}\right.$$

$$\text{mean}\quad x=0{\cdot}52$$

This equality of the values of x proves that the primitive circuit is equivalent to 0^{m}, 52 of the length of the wire submitted to experiment; and, if we calculate in reality, the deviations that ought to be observed, admitting this value of x or this *resistance* of the element itself and of the band of the compass, we obtain the following table.—

Comparison between the observed and calculated deviations.

Length of the circuit added to the element.	Total length of the circuit adopting 0·52 for the element	Deviations observed.	Deviations calculated.	Differences.
0	0·52	64°30′	64°30′	0
1	1·52	35·15	35·32	—17′
2	2·52	24·00	23·51	+ 9
4	4·52	13·40	13·37	+ 3
8	8·52	7·30	7·18	+12
16	14·52	3·40	3·47	— 7

The differences between the observed and calculated deviations are so completely within the limits of the errors of observations that it is impossible not to look upon the principle on which the calculations have been made as perfectly rigorous.

ELEMENT A — *Copper wire.*

Lengths added to the length of the element.	Deviations observed.	Tangents of the deviations or intensities.	Lengths of the element or resistance
0	62° "	1 880	" "
5	40·20'	0·849	4^{m}11
10	28·30	0 543	4·06
40	9·45	0·172	4·03
70	6· "	0·105	4·14
100	4·15	0 074	4·09
		Mean....	4·08

Comparison between the observed and calculated deviations.

Total lengths.	Calculated deviations.	Observed deviations.	Differences.
4'08	62° "	62 "	"
9·08	40·18'	40·20'	+2'
14·08	28·41	28·30	—11
44·08	9·56	9·45	—11
74·08	5·57	6· "	+ 3
104 08	4·14	4·15	+ 1

ELEMENT B — *Copper wire.*

Length added to the length of the element.	Deviations observed.	Tangents of the deviations or intensities.	Length of the element or resistance.
0	54°30°	1·400	" "
5	34· "	0 674	4^{m}64
10	24·20	0·452	4·77
40	8·30	0·150	4·80
70	5·10	0·090	4·81
100	3·40	0·064	4·71
		Mean....	4·75

Comparison between the observed and calculated deviations.

Total lengths.	Calculated deviations.	Observed deviations.	Differences.
4 75	54°30'	54°30'	"
9·75	34·15	34· "	—15
14·75	24·20	24·20	"
44·75	8·27	8·30	+ 3
74·75	5· 6	5·10	+ 4
104 75	3·37	3·40	+ 3

Many other series made with wires of different kinds led to the same result; and we have derived the following general law from them.

The intensity of the current produced by a single element is in an inverse proportion to the real length of the circuit.

Some analogous experiments have served to demonstrate that the resistance of the element or the primitive length of the circuit is expressed by lengths, which are proportional to the section, and the conductibility of the wire which composes the apparent length of the circuit.

The result of which is, that the intensity of the current produced by one element is expressed by the general formula,

$$\frac{csr + cs}{csr + l};$$

c representing the conductibility of the circuit, s its section, l its apparent length, and r the resistance of the element for a circuit where conductibility and section are taken for unity.

Since the intensity of the current observed with the apparatus is in an inverse proportion to the real length of the circuit, we may obtain this important conclusion, that the current produced by a single element is *capable of a constant electro-dynamic effect*: for the effect that is observed on the magnetized needle is produced only by a certain fraction of the real length of the circuit, but if we increase it tenfold, for example, the real length of the circuit, this fraction becomes ten times as small, at the same time that we obtain one tenth of the intensity; hence it is evident, that in the two cases we should obtain *equal intensities*, if we could, under the same circumstances and conditions, cause the total length of the circuits to act on the needle.

This is quite a fundamental principle in theory, since it shows that the unknown modification which constitutes the current may be assimilated to a quantity of motion which must essentially remain constant whatever may be the extent of the mass in which it is propagated. Thus, if the two poles of a voltaic element be connected by a wire of one metre, or by one of 1000 metres, neither more nor less electricity passes in the one case than in the other: the quantity which passes remains always constant, and depends solely on the quantity furnished by the element itself, or, generally, by the electric source, whatever it may be.

4. *Derived currents.* When two points of a circuit traversed by any current are touched with the two extremities of a metallic wire, it is evident that at the two points touched, which are called the *points of derivation*, the current must be

divided, one portion continuing to pass in the circuit, as before, and the other portion taking the direction of the wire to traverse it throughout its length; this latter portion is called *derived current*; the portion which traverses the original circuit between the points of derivation, is called *partial current;* the current itself which passes in the circuit before and after the points of derivation, is called *principal current;* and the current which passed before the derivation was made, is called *primitive current.*

We measured by means of the compass of tangents and the compass of sines, the intensities of the derived, partial, and principal current, and the results of experiments have brought us to the following general laws:

1. As soon as a derivation is made, the intensity of the primitive current is increased; thus the principal current is always stronger than the primitive.
2. The intensity of the derived current is proportional to the distance of the points of derivation.
3. At an equal distance it is in an inverse proportion to the section and conductibility of that portion of the circuit in which the derivation is made.
4. The sum of the intensities of the partial and derived currents is always equal to the intensity of the principal.

From these laws, and those which have been established above, result the following formula, to express the intensities x, y, and z, of the principal, partial, and derived currents:

$$x = T.\frac{(pk+1)}{pk+1-n},$$

$$y = T.\frac{pk}{pk+1-n},$$

$$z = T.\frac{1}{pk+1-n},$$

T. is the intensity of the primitive current. n, the fraction which expresses the ratio between the distance of the points of derivation, and the total length of the circuit. k, the ratio of the length of the wire which makes the derivation and the distance of the points of derivation. p, the ratio between the sections of the circuit and the wire of derivation, these sections being reduced, if the conductibility be different.

5. ***Experiments with a pile of six elements and general formula for the intensity of piles.*** Having arranged six elements similar to those employed in the preceding experiments, their intensity and individual resistance was determined by the means already mentioned. The following table contains the result of the experiments.—

Numbers of elements.	Length added to the element.	Observed deviations.	Tangents, or intensities.	Resistances.
1	0	69°00'	2·600	0m 00
	5	43·20	0·943	2·85
	10	30·00	0·577	2·85
	40	11 00	0·194	3·20
				Mean 2·97
2	0	66·30	2·300	0·00
	5	43·00	0·933	3·41
	10	29·40	0·570	3·35
	40	10·40	0·188	3·55
				Mean 3·44
3	0	67·40	2·434	" ""
	5	42·30	0·916	3·02
	10	29·40	0·570	3·05
	40	10·20	0·182	3·23
				Mean 3·10
4	0	67·00	2·355	" ""
	5	42·30	0·909	3·19
	10	29·40	0·570	3·19
	40	10·20	0·182	3·35
				Mean 3·25
5	0	68·00	2·475	" ""
	5	43 20	0·943	3·08
	10	30 30	0·589	3·13
	40	11·00	0·194	3·40
				Mean 3·21
6	0	64·00	2·050	" ""
	5	41·00	0·869	3·68
	10	28·40	0·548	3·64
	40	10·00	0·176	3·75
				Mean 3·69

Thus, these elements have nearly the same force; although that of the sixth is sensibly less.

Immediately afterwards, we arranged the whole of these elements to make a pile of six pairs.

The intensity of this pile was such that it reddened a platina wire 1-4th. of a millimetre in diameter, and 20 centimetres long.

The current that it could produce was then made to traverse the compass of tangents, and we obtained the following results with the copper wire which was used to determine the individual resistances.

Length added.	Deviations observed.	Tangents of the deviations.	Resistances.
0	68°30'	2·538	„ „
5	63·20	1·991	18·20
10	58 30	1·632	19·03
40	39·00	0·810	18·01
70	28·00	0·532	18·56
100	21·30	0·394	18·38
		Mean	18·43

When the six elements are added end to end to form a pile of six pairs, the current produced by the first element has no longer to traverse only the conductors of the apparatus, but it must also traverse the five other elements, and be weakened in proportion to the length of the wire which represents the resistance of these elements; consequently to ascertain its individual intensity when it forms part of the pile, we must calculate it from the real length of the new circuit it traverses; it is the same for all the other elements. In making these calculations and adding thereto the individual intensities of the six elements thus calculated, we discover all the observed intensities, whether for the pile alone, or for the different circuits composed of the pile and of 5^{m}, 10^{m}, 40^{m}, 70^{m}, 100^{m}, of wire.

Generalising these results, we obtain the following formula which expresses the intensity of any pile by means of the individual intensities of its elements.

$$\frac{r_1 t_1 + r_2 t_2 + \ldots\ldots\ldots + r_n t_n}{r_1 + r_2 + \ldots\ldots r_n - (n-1)a + l};$$

$r_1, r_2, \ldots\ldots r_n$ are the resistances of the elements $a = 0^{m} \cdot 26$.

$t_1, t_2, \ldots\ldots t_n$ the individual intensities.

a, the length of wire which represents the resistance of the compass. In the preceding experiment, $a = 0^{m} \cdot 26$. l, the length of wire added to the circuit of the pile. Thus the principles demonstrated for one element, apply to a pile composed of any number of pairs, and, at the same time, we may say, with truth, that *the intensity of a pile is in an inverse proportion to the length of the circuit, and that a pile is capable of a constant electro-dynamic effect,* whatever may be the length, section, and conductibility of the circuit that its current has to traverse.

This result explains what is called the tension of the pile; for, if an element be made with large surfaces, whose intensity and resistance are represented by T and R, when the current of this element has to traverse a length l, of wire as above, its intensity will be

$$\frac{\mathrm{R\,T}}{\mathrm{R}+l};$$

but, it will be very easy to give this element sufficient surface, that in making $l=o$, its intensity, T, would be greater than that of a pile composed of n smaller elements; but even when a very small value is given to l, its intensity is generally weakened in an enormous proportion, while the intensity of the pile remains almost the same. Thus in this respect to compare an element with a pile, T and R must at the same time be very great, which, perhaps, is realised by some very energetic chemical actions, and also fulfilling some other conditions.

6. *Experiments with several elements placed pole to pole.* To determine the law of the intensity of given currents, by several elements placed pole to pole, and, consequently, forming a pile with great surface and a single element, we have connected the two positive and the two negative poles of the elements, A and B, whose individual intensities have been already ascertained (page 96), and of which the following are the results:

ELEMENTS A *and* B, *placed pole to pole.*

Lengths added	Deviations.	Tangents.	Resistances.
0	73°00	3·270	" "
5	45 00	1· "	2·20
10	30·30	0·589	2·20
40	9·30	0·167	2·15
70	5 40	0·100	2·20
100	4·00	0·070	2·20
		Mean	2·20

In this experiment, the element B ought to be considered as making a derivation in the current produced by the element A, and reciprocally the element A makes a derivation in that produced by B. Thus the intensity of the current which traverses the compass is the sum of two partial currents. By calculating them according to the formula given in the article on derived currents, we obtain the results which are laid down in the following table, and which are compared with the results of observation.

Lengths added.	Deviations observed.	Tangents.	Intensities calculated & given, by the sums of the partial currents.		Differences.
0	73°00'	3·276	1·400 1·880	3·28	—3'
5	45·00	1·000	0·425 0·577	1·002	—5
10	30·30	0·589	0·253 0·337	0·590	—3
40	9·30	0·167	0·070 0·095	0·165	+7
70	5·40	0·0992	0·0432 0·0577	0·1009	—6
100	4·0	0·0700	0·0303 0·0407	0·071	—3

Thus in this case, the individual currents of the two elements join and superpose themselves in some manner without any particular modification. This result is remarkable, in more than one respect: for it shows that when a wire is traversed by a current of a certain tension, it is not less fit to receive another current, even though it may be produced by a source of less tension; which gives a fresh proof that the currents are assimilated to certain quantities of motion, and that it is not necessary to consider electric conductors as a species of tube giving passage to a fluid, and offering as much more resistance as they are increased in length, so that the fluid diminishes in velocity or quantity, and is obliged either to reflow towards the source, or at least to be accumulated in a greater proportion.

Experiment has given us this general formula to express the intensity of the current produced by several elements united pole to pole:

$$\frac{r_1 r_2 r_3 \ldots r_n (t_1 + t_2 + \ldots t_n)}{r_1 r_2 \ldots r_n + l (r_2 r_3 \ldots r_n + r_1 r_3 \ldots r_n + \&c.)};$$

for the case in which all the elements would have the same intensity and the same resistance the formula becomes:

$$\frac{n r^n t}{r^n + n l r^{n-1}} \quad \text{or} \quad \frac{n r t}{r + n l};$$

hence for $l = o$, the intensity is $n\,t$.

But immediately we add to the circuit of the elements themselves a length l of a wire equivalent only to n times the resistance r of one of the elements, the intensity becomes:

$$\frac{n t}{1 + n^2};$$

that is to say, that in this case it diminishes very rapidly in proportion as we increase the number of the elements, so much that the intensity of a single element would be almost ten times greater than that of a pile of ten elements.

7. The summary of the experiments contained in this memoir lead as a last result to these two general laws, which are of a remarkable simplicity.

1.—An electric source is capable of a constant electro-dynamic effect, whatever may be the nature and extent of the metallic circuit traversed by the current it produces.

2.—When we connect several electric sources, their effects join or superpose themselves without modification.

Experiments, already very numerous, authorize us to think also that an electric source is capable of producing a constant quantity of heat, and that it is possible to estimate, by quantities of heat, or by quantities of ice melted, the *quantities of electricity* given by the piles, the chemical reactions, or in general by the electric sources.

XIV. *Observations on Sulphurous Ether, and Sulphate of Etherine (the true Sulphuric Ether). By* R. Hare, *M.D., Professor of Chemistry in the University of Pennsylvania.*

It is known that when two parts, by weight, of sulphuric acid are distilled with one of alcohol, a yellow sulphurous liquid is obtained. Berzelius alleges, that when this liquid is exposed in an exhausted receiver over sulphuric acid and hydrate of potash, an oleaginous liquid remains, which he designates as "*oil of wine containing sulphuric acid, or heavy oil of wine.*"

This oil is, by the same author, described as being heavier than water, as having a penetrating aromatic odour, and a cool pungent taste, resembling that of peppermint. It is, in fact, the liquid which Hennel first analysed as oil of wine, without, at the same time, mentioning the process by which it was procured. No doubt the difference between it and that procured by Boullay and Dumas, was, in some degree, the cause of the discordance between his observation and theirs. According to Hennel, the oil of wine consists of an atom of sulphuric acid, and an atom of hydrocarbon: $\dddot{S}+4C+4H$. By the last mentioned appellation, this skilful chemist designates a compound, consisting of four atoms of carbon, and four of hydrogen.

Serullus represents the oil in question as consisting of two atoms of the acid, two of hydrocarbon or etherine, and one of water.

To the hydrocarbon of Hennel (4CH), as the common base of all the ethers, excepting those lately alleged to have mytheline for a base; the name of etherine has been given; so that the heavy oil of wine may be called the sulphate of etherine: or, according to the formula of Serullas, $2\dddot{S}E+H$, it is a hydrous sulphate of etherine. It is in fact, the only compound to which the name of sulphuric ether can be applied with propriety. The yellow liquid out of which it is procured, as above stated, may be designated as the ethereal sulphurous sulphate of etherine.

Another oil, lighter than water, resulting from the distillation of the ethereal sulphurous sulphate of etherine, from hydrate of lime, or from potash, is described by Berzelius as oil of wine exempt from sulphuric acid. Of this the odour is represented as disagreeable; and, though nothing is said of its taste, it is to be presumed that it differs from the heavy oil of wine in this respect, as well as in its odour and specific gravity.

Thènard alleges, that when the heavy oil of wine is heated with water for some time, a liquid swims on the water, which, if refrigerated by ice, will, within twenty-four hours, deposit crystals. The mother liquid he calls light oil of wine, while to the crystals he gives the name of concrete oil of wine. Hennel mentions his having obtained a similar product by the reaction of oil of wine with water, or an aqueous solution of potash; and treats the crystalline matter as the base of the heavy oil of wine, deprived of its acid; or, in other words, as his "hydrocarbon;" or, as above mentioned, etherine.

Considering how much has been written on this topic, I am surprised that I have met with no statements respecting the reaction of ammonia with the above mentioned ethereal sulphurous sulphate of etherine.

Since the year 1818, I have been accustomed to saturate the acid in that liquid by ammonia. The residue being rendered very fragrant, and entirely freed from its sulphurous odour, by admixture with about twenty-four parts of alcohol, was found to constitute an anodyne, possessing eminently all the efficacy of that, so long distinguished by the name of Hoffman. When the residue, remaining after saturation with ammonia, was distilled in a water bath, ether came over, and left an oil, which I was accustomed to consider as the oil of wine.

I had observed that, in the process above mentioned, there was a striking evolution of vapour, which seemed irreconcilable with the received opinion of the re-agents employed. Since the affinity between the ammonia and sulphurous acid is energetic, it did not appear to be reasonable that a copious escape of the one should be caused by its admixture with the other; and it was no less improbable that the vaporization of hydric ether, in its natural state, could take place at temperatures so much below its boiling point as those at which this phenomenon was noticed. In order to ascertain the truth, I luted a funnel, furnished with a glass cock and an air tight stopple, into the tubulure of a retort, of which the beak was so recurved downwards as to enter and be luted into the tubulure of another retort. The beak of the latter passed under a bell over water.

Both retorts were about half full of liquid ammonia, and surrounded with ice. The apparatus being thus arranged, about a thousand grains of the ethereal sulphurous sulphate of etherine were poured into the funnel, and thence gradually allowed to descend into the ammonia in the first retort. Notwithstanding the refrigeration, much heat was perceptible, and a copious evolution of vapour, which, passing into the second retort, was there absorbed or condensed, none being observed to reach the bell glass. At the close of the operation, hydric ether, holding oil of wine in solution, floated upon the ammonia in the first retort, and pure ether, of the same kind, floated on the ammonia in the second.

The ammonia in both retorts gave indications of the presence of sulphurous acid, on the addition of sulphuric acid. From these results, I inferred that a chemical compound of sulphurous acid and hydric ether formed the principal portion of the yellow liquid, and might be separated by distillation. Accordingly, by means of retorts arranged and refrigerated as above described, I procured a portion of sulphurous ether, which boiled at 44°, and which, when agitated with ammonia in a bottle, produced so much heat and consequent vapour, as to expel the whole contents in opposition to the pressure of my thumb. By employing the same distillatory apparatus, I subjected 2150 grains of the ethereal sulphurous sulphate of etherine to distillation, and obtained 726 grains of sulphurous ether, which boiled as soon as the frigorific mixture was removed from the containing retort. This being redistilled, as in a former experiment, so as to receive the product in ammonia, left in the retort five grains of oil of wine. The resulting ammoniacal liquid, saturated with chloride of barium in solution, gave a precipitate, which, agreeably to the table of equivalents, contained 356 grains of sulphurous acid.

The residue of the 2150 grains of ethereal sulphate being subjected to distillation, raising the temperature from 95°, the point at which it had been before discontinued, to 140°, the product obtained by means of a refrigerated receiver, weighed 602 grains. This was, of course, inferior in volatility to the first portion distilled; and, when redistilled, it was found to contain a small quantity of oil of wine. In fact, it appears, the boiling point of the ethereal sulphurous sulphate rises, not only as the ratio of the sulphurous acid lessens, but also as the proportion of oil of wine augments.

The residual liquid being exposed to the heat of a water bath at 212°; a very fragrant, and well flavoured oil of wine was evolved, and floated upon a quantity of water acidulated by sulphuric or sulphovinic acid.

Agreeably to another experiment, 1750 grains by weight, of the ethereal sulphurous sulphate of etherine, after washing with ammonia, gave 869 grains of an ethereal solution of oil of wine. This being subjected to distillation by a water bath raised gradually to 190°, there remained in the retort 148 grains of oil, beneath which there were a few drops of acidulated water. Agreeably to the result of several experiments, the ethereal sulphurous sulphate of etherine yields about half its weight of the ethereal solution of oil of wine. The quantity is always somewhat less than half when weighed; but the deviation is not greater than might be expected to result from the loss by evaporation, and the diversity of refrigeration employed in the condensation of the ethereal sulphurous sulphate, during the process by which it is evolved.

Under the expectation of procuring a sulphurous ether of a still higher degree of volatility, I associated with the apparatus usually employed in the process for generating hydric ether, a series of tubulated retorts, of which the beaks were recurved downwards in such a manner that the beak of the first communicated with a perpendicular tube, passing through an open-necked cylindrical receiver, so as to enter the tubulure of the second retort, of which the beak was in like manner inserted into a tube passing through a receiver in a third retort, and this communicated in like manner with a fourth retort. The second, third, and fourth retorts, and the tubes entering them, were all refrigerated, the first with ice, the second with ice and salt, and the third with ice and chloride of calcium.

By these means, on subjecting to distillation in the first retort 48 ounces of alcohol of 830, and a like weight of sulphuric acid, besides the ethereal sulphurous sulphate of etherine usually resulting from the process, and condensing in the first receiver; it was found that in the other retorts severally, there were liquids of various degrees of volatility. That in the last boiled at 28°, but the boiling points rose gradually as the quantity of the residual liquid diminished.

In order to ascertain the nature of the sulph-acids abstracted from the ethereal sulphurous sulphate of etherine by the ammonia employed, chloride of barium was added in excess to the resulting ammoniacal solution, until no further precipitate would ensue. The liquid having been rendered quite clear by filtration, soon became milky. By evaporation to dryness, and exposure to a red heat, a residuum was obtained which proved partially insoluble in chlorohydric acid, and by ignition with charcoal, yielded sulphide of barium. It appears, therefore, that a hyposulphate of barytes existed in the liquid after it was filtered; as I believe that the hyposulphuric acid is the only oxacid of sulphur which is capable of forming with barytes a *soluble* compound, susceptible, by access of oxygen, of being converted into an insoluble sulphate, and precipitating in consequence.

It must be evident from the facts which I have narrated, that the yellow liquid obtained by distilling equal measures of sulphuric acid and alcohol, consists of oil of wine held in solution by sulphurous ether, composed of nearly equal volumes or weights of its ingredients; also, that the affinity between the *ether* and the acid is analogous to that which exists between alcohol and water. The apparent detection of sulphuric acid in the ammonia, justifies a surmise, that the etherine distils in the state of a hyposulphate, which subsequently undergoes a decomposition into sulphurous acid and sulphate of etherine.

The liquid above alluded to, as resulting from the saturation of the ethereal sulphurous sulphate of etherine by ammonia, and distillation by means of a water bath gradually raised to a boiling heat, is a very fragrant variety of oil of wine. It differs from that described by Berzelius as the heavy oil of wine of Hennel and Serullas, in being lighter and containing less sulphuric acid. I have a specimen exactly of the specific gravity of water, and have had one so light as to float on that liquid. The oil of wine obtained by ammonia approximates, in its qualities, to the variety which Thènard describes as light oil of wine. The presence of sulphuric acid in a definite or invariable ratio does not appear requisite to the distinctive flavour or odour of oil of wine.

The heavy oil of wine treated by Hennel as sulphate of hydro-carbon, $2\dddot{S}+4CH$; and by Serullas as a hydrous sulphate of etherine $4CH+2\dddot{S}+\dot{H}$; I have obtained, as above mentioned, by exposing the ethereal sulphurous sulphate of etherine, in vacuo, over the hydrate of lime, or potash, and sulphuric acid. This variety sinks in water, being of the specific gravity of 1·09 nearly; is of a deeper hue than the other, and of a smell less active, with a taste somewhat more rank. A specimen of oil thus obtained being subjected to the distillatory process, a portion came over undecomposed, leaving in the retort a carbonaceous mass. 14 grains of the oil which had not undergone distillation, and a like portion of the distilled oil, were severally boiled in glass tubes with nitric acid until red fumes ceased to appear; about 28 grains of pure nitre were added to each, some time before the boiling was discontinued. The resulting liquid was in each case poured into a platina dish, boiled dry, and afterwards deflagrated by a red heat. The residual mass being subjected to water, the resulting solution was filtered, an excess of nitric acid added, and then nitrate of barytes in excess.

The precipitate obtained from the distilled oil, weighed, when dry, only nine and five-eighths grains, while that procured from the oil which had not been distilled, amounted, under like circumstances, to fourteen and one-eighth grains. Ten grains of another portion, left for some time over liquid ammonia, yielded only seven-eighths of a grain of sulphate.

About a drachm of Hennel's oil of wine was subjected to distillation with strong liquid ammonia; fourteen and a half grains came over, retaining the appropriate fragrance and flavour. This yielded, by the process above described, only two grains of sulphate of barytes. After all the water and ammonia had distilled, the receiver was changed, and fourteen grains of oil, devoid of the fragrance and flavour of the oil of wine, were obtained. This yielded one and one-eighth grains of sulphate. A carbonaceous mass, replete with sulphuric acid, remained in the retort.

Hennel states that when oil of wine was heated in a solution of potash, an oil was liberated which floated upon water, having but little fluidity when cold; and which, in some cases, partially crystallized. When gently heated, it became clear, and of an amber colour. The vapour had an agreeable, pungent, aromatic smell. This oil must have been pure etherine.

It is not improbable that this oil, which may be considered as devoid of sulphuric acid, is more or less liberated in evolving oil of wine, according to the nature of the process employed; and that the oil alluded to by Thènard, and those procured by me by simple distillation, ebullition, or distillation with ammonia or potassium, are mixtures of the etherine with its sulphate in various proportions. As it is well known that the odour of the essential oils is rendered more active by dilution, the livelier smell of the solutions may be consistent with a diminished proportion of the odoriferous matter.

Oil of wine cannot be distilled per se without partial decomposition, which does not take place below the temperature of 300. When subjected to the distillatory process, over potassium, at a certain temperature, a brisk reaction ensued, and the oil and metal agglutinated into a gelatinous mass. By raising the temperature the mass liquefied, and a colourless oil came over, which retained the odour of oil of wine. Meanwhile some of the potassium remained unchanged, and appeared within the liquid in the form of pure metallic globules. On pouring into the retort a portion of nitric acid in order to remove the caput mortuum, ignition took place from the presence of the potassium.

XV. *Researches in Electro-dynamics, Experimental and Theoretical. By* William Sturgeon, *Lecturer on Experimental Philosophy, at the Honourable East India Company's Military Seminary, Addiscombe.*

On the production of secondary electric currents in a metallic spiral, independently of opening and shutting the Battery circuit: or, of giving motion to either the primitive or secondary conducting wires.

Secondary electric currents have hitherto been produced by two distinct processes, very different from each other. One of these processes requires motion of either the *primitive's* or *secondary's* conducting wire, or of both at the same time: and the other requires the sudden opening or shutting of the primitive battery circuit.

The phenomena exhibited by these two processes of excitation, though perfectly identical, for awhile appeared untraceable to any definite cause; and had not the laws of magnetic electricity been previously developed, and obtained an intelligible aspect, it is probable that the doctrine of secondary electric currents had not yet been very well understood. But by keeping in view the laws which govern magnetic electrical excitation, it was not difficult to trace secondary electric currents to a similar source of production. I have endeavoured, in former papers in these Annals,* to simplify these laws as far as they appear, to me, to be susceptible: and, if I mistake not, they are now, as far as they proceed, in as intelligible a form as any system of laws that has hitherto appeared within the precincts of experimental science.

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* See Vol. I. page 198, 251. Application of the theory, &c., 266.

At the time I was arranging the experimental problems for solution by the application of my theory of magnetic electricity, it occurred to me that, if the views which I had taken were correct, secondary electric currents ought to be produced independently of either a motion given to the conducting wires, or, of opening and shutting the battery circuit. For since the phenomena of secondaries depend upon the motions of the electro-magnetic lines of the primitive battery current, or its conductor; and as those lines may be put into motion in a variety of ways without any sudden disruption of the primitive circuit, by merely *varying the degree* of the battery's action, it only required the selection of the easiest plan of accomplishing the latter point in order to proceed at once to an experiment, which, if successful, promised to be more important than any other, yet on record, in supporting the doctrine of secondary electric currents, which I was then presenting to the notice of philosophers.

The first apparatus employed in this investigation consisted of two concentric coils of wire, one above the other, on the same reel; a galvanometer; and a voltaic battery of a single pair. The copper and zinc plates of the battery were each six inches high and four inches broad, with conducting wires attached by solder, to connect them with the inner coil. The trough which held the plates and acid solutions was twelve inches long and sufficiently wide for the free motion of either plate parallel to its own plane, from one end to the other. The zinc plate was fixed, by slips of wood, close, and parallel, to one end of the trough, which was filled to a little less than four inches deep, with very dilute nitrous acid. The ends of the inner coil-wire being connected with the copper and zinc plates, and the ends of the outer coil-wire with the galvanometer, the experiments were carried on in the following manner.

On plunging the copper plate into the acid solution at the distance of six inches from the zinc, the battery current rushed through the inner coil, and the galvanometer needle was deflected by the secondary produced in the outer coil, according to the law which governs the excitation by a *distention* of the electro-magnetic lines of the battery current. When the needle had come to rest again, the copper plate was advanced towards the zinc, until within half an inch of the latter metal. The needle, during this motion of the copper, was again deflected in the same direction as before, indicating a flow of a current through the galvanometer, and consequently through the outer coil. The copper plate was now made to recede from the zinc to nearly the opposite end of the trough, during which the needle deviated on the other side of the meridian, indicating the flow of a secondary current in the opposite direction to the former. By timing the motions of the copper plate, to and from the zinc, to the motions of the needle, the latter was made to sweep over an arc of 40° on each side of the meridian.

The results of these experiments were in strict accordance with my anticipations, and were perfectly satisfactory as far as regards the production of secondary electric currents by periodic vicissitudes in the energy of the primitive. To understand the mode of excitation of these secondaries, it is only necessary to bear in mind that the electro-magnetic lines of the primitive are never stationary only whilst that current is of an invariable energy, and that every vicissitude of the latter is attended by a corresponding motion of the former. When the power of the battery current is exalting, by the gradual approach of the copper to the zinc, the electro-magnetic atmosphere, of the inner coil, is distending, and gives the exciting impressions to the exterior coil. But as the copper plate recedes from the zinc, the battery current becomes gradually feebler, and a corresponding *collapsion* of its magnetic lines takes place. Exciting impressions are now given to the exterior coil, producing a secondary current in the opposite direction to the former.

Notwithstanding these satisfactory results, the battery I employed was by no means well adapted for the purpose of the experiment. The wave of acid solution, produced by the motions of the copper plate, would frequently flow over the side of the trough, and cause a nuisance on the table it was placed on; besides which, the trough itself was too cumbersome for a lecture-table apparatus: and the power of the secondaries, by this process, were not sufficient for prompt illustration to an extensive class.

The battery I now use for exhibiting secondary electric currents, by varying the energy of the primitive, consists of two long narrow concentric cylinders of copper and zinc; one of each metal: the zinc, being the inner one, is covered with calico to prevent its touching the copper, in which it is fixed by wedges or otherwise. To the top of each cylinder is soldered a long copper wire, for connexions with the inner coil of the reel. The cylinders are placed in a glass jar, ten inches high and about two in diameter.

When the connexions are properly made, the inner coil with the battery, and the outer one with the galvanometer, the jar is to be nearly filled with acid solution. This done, the battery action is lessened by lifting the metallic cylinders gently upwards, exposing a less and less surface to the acid solution. By this means a secondary, of considerable deflecting power, is brought into play. The opposite secondary is produced by letting the metal down again, and thus augmenting the energy of the primitive. And by proceeding in this manner, moving the metals up and down in the acid solution, in correspondence with the motions of the needle, the latter is soon made to sweep a very extensive arch of the card. The metals, by this process, are never permitted to quit the acid solution, and, consequently, the secondaries are not produced by opening and shutting the circuit: neither are they *momentary* only, as by those processes: which is a great advantage in the production of deflections, and also in that of decompositions.

Another method of producing secondary electric currents, without opening and shutting the primitive's circuit, is by means of the magnetic electrical machine described in the last number of these "Annals:" or with any other magnetic electric machine, to which a similar discharging apparatus is attached. The *polar springs* of the machine (or the *polar cells* as described in article one, of this volume), which are connected with the semi-wheels, fig. 6, plate I. are united, by wire, to the extremities *z*, c, of the inner coil wire fig. 125, plate XV. vol. I: and a galvanometer, or other apparatus, to the extremities *r*, *r*, of the outer coil wire. By turning the wheel of the machine, the usual deflections of the needle, and other phenomena, are produced by the secondary current.

When *interruptions* are made in the primitive, as in the method of producing shocks by magnetic electrical machines, and the cylinders *r*, *r*, fig. 125, are held in the hands, shocks, still more powerful than those given by the machine, are experienced. The bundle of iron wires has here its singular effect of increasing the power of the shocks.

A series of experiments, with this apparatus, will shortly be offered to the notice of the Electrical Society. I have, therefore, in this place, recorded only a brief statement of these novel facts, which can hardly fail to produce an important change in the construction of magneticelectrical machines.

XVI. *Notes on Chemistry, &c.* *By* J. W. BAILEY, *acting Prof. Chem. &c, U. S. Mil. Academy, West Point,*[*]

1. *Substitution for frogs in Galvanic Experiments.* Persons who may have occasion to repeat Galvani's experiment on the legs of frogs, will doubtless be pleased to hear of some substitute, which will enable them to dispense with

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† From Silliman's American Philosophical Journal.

the disgusting operation of cleaning, skinning, &c. which are necessary before the legs of a frog can be used. I find that a leg of the common grasshopper may be made to exhibit the muscular contractions; and as it appears to be easily affected by electricity, can frequently be obtained when frogs cannot; can be prepared at a minute's notice; and retains its irritability for five or ten minutes, it forms an excellent substitute.

The method of preparation consists merely in removing with a sharp penknife, from each side of the thick part of one of the leaping legs, a portion of the skin, so as to expose the flesh; then by laying the underside of the leg upon a small piece of moistened zinc, and bringing the piece of copper in contact with the flesh exposed on the upper side, no motions will be observed until the copper also touches the zinc, when quick movements or jerks of the lower part of the leg will be seen, each time that contact is made. In fig. 14, plate III, *z* is the zinc, *c* the copper, A, B, the part of the leg which will be observed to move.

2. *Washing Bottles.* The admirable contrivances of Berzelius and Gay Lussac, by means of which a substance to be washed upon a filter, may be supplied with water as fast as it is required are not, I believe, so well known in this country, as their merits entitle them to be. I have used them with so much satisfaction, that I am induced to send the accompanying drawings and description; believing that they will be useful to some persons, who may be engaged in analysis or pharmacetical preparations, and to whom they may be unknown.

Berzelius remarks (Traité, tome viii, p. 270.) "Few modern instruments are so valuable to the practical chemist as these simple washing bottles, since by means of them, the washing may be continued during all the time the operator may be obliged to be absent, and during his presence it requires no particular attention," except to see that channels do not form, which may lead the water off too rapidly.

The bottle used by *Berzelius* is fitted with a perforated cork, in which is inserted a tube of the form and dimensions represented by fig. 15, plate III. A, B, is a tube drawn out below and turned up, terminating in a small orifice, C. The small piece of quill tube, D. communicates with A, B, by the orifice E. A person tolerably skilled in the use of the table blow pipe, can make one of these in ten minutes.

The tube thus prepared is fixed, by means of a perforated cork, in a bottle containing water, and reversed over the filter, as in fig. 16, so that the orifice, C, is placed just below the upper surface of the liquid in the filter. When this liquid falls below a line, F, G, the weight of the column of water, from E to F G, overcomes the capillary attraction, which retained a portion of water in the tube, D; air will, consequently, enter at E, and water escape at C; thus the water in the filter will be kept at the level required. The pure water that issues at C, displaces the solution below it and the washing goes on in the most rapid and best manner possible.

Gay Lussac's arrangement is equally simple, and may be prepared with even more facility than the preceding. The water is contained in a wide mouthed bottle, A B, fig. 17, which is closed by a cork, through which passes a syphon with equal legs, C, D, and a straight tube, E, F, whose lower orifice is a little above the level of the opening of the syphon. To facilitate the entrance of the air by means of this tube, it is cut off obliquely at bottom, as shown in the figure. The end, D, of the syphon being plunged into the liquid in the filter, the water will commence running out, while air enters, bubble by bubble, through the tube, E F. The water in the filter will not rise above the level of the orifice, F.

The best shape of the syphon is shown in figure 18, in which the exterior opening is turned upwards. No practical chemist should be without one or the other of the above arrangements.

XVII. ROBERT WERE FOX'S *Observations on* W. J. HENWOOD'S "*Rejoinder*" *inserted in Sturgeon's Annals of Electricity, No. 7, p.* 79.

Owing to some delay on the part of the Booksellers, I did not receive the last number (7) of the "Annals of Electricity" till long after the usual time; but I hope, notwithstanding this, that I may still not be too late for the next month's publication.

It is not, perhaps, necessary to comment on the tone of W. J. Henwood's "*rejoinder*" beyond referring to my short notice in Annals, No. 6, p. 507, to which he applies the terms which he has used. If, as I have therein stated, his experiments were different from those which I had described,* what ground had he for objecting to the correctness of the latter? I could not well say more than I did without showing, which I was reluctant to do, that his experiments, marked *a*, *b*, *c*, and *d*,† were not even *electrical* in their character, no circuit having been formed. In fact, the bodies intended to act on each other were insulated by being placed in "*two* contiguous cells of a *glazed* earthenware trough."[*]

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* Vol. 1. p. 133, of Annals of Electricity.

† Ibid. p. 225, 226.

In the only remaining experiment which he has reported, he employed the sulphuret and bisulphuret of copper, *both* of which were plunged into water, holding sulphate of copper in solution; whereas I placed the bisulphuret, and not the sulphuret, in a similar solution: and, in this experiment, the effect did not penetrate much beyond the surface. It was only when the bisulphuret of copper was used in connexion with *zinc*, or some other electro-positive metal, that the former was changed into the sulphuret to some depth, and copper was deposited on it from the solution in brilliant crystals.[†]

Having mislaid the notes of my results, I cannot just now say precisely how much the copper pyrites was reduced in weight when the crust of sulphuret was in great part removed, but I think that pieces which originally weighed 400 or 500 grains, lost at least eight or ten per cent. It would not, of course, have answered to weigh the ore till after the copper *deposited* upon it was removed, and much of the sulphuret came off at the same time in consequence of its adhering to the former and being friable: whereas this was not the case with the pyrites which was comparatively hard and compact.

W. J. Henwood speaks of there being "*chemical objections*" to the change in question without explaining what they are, whilst I, on the contrary, apprehend that it may be easily accounted for on chemical principles.

That the processes of *deposition* and *decomposition* may simultaneously go on, in the cases of compound bodies, such as copper pyrites, even at the electro negative pole, I apprehend that no one who understands electro-chemistry will deny, and that they do, in the instance before us, my experiments unequivocally prove; and if so, they differ from those made by Becquerel, on W. J. Henwood's own admission.[‡] Indeed, I do not believe that the former ever employed pieces of copper pyrites or of any other ores to produce voltaic action, although the latter implies that he did.*

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[*] Vol. I. p. 225, of Annals of Electricity.

[†] The specimen of artificial copper ore which we have received from Mr. Fox, has appeared to us to be satisfactory evidence of the change which had taken place from the bisulphuret to the sulphuret of copper. The fragments which had come off, with the exception of metallic particles, appeared to consist of the sulphuret only. Edit.

[‡] Edinburgh Philosophical Journal, vol XXII. p. 281. Annals of Electricity, vol. I. p. 227. Mining Review, No. 10. New Series, p. 22 9.

With respect to the non-occurrence of metallic tin in our copper lodes, he does not seem to be aware that if it was once deposited under water, in the presence of highly electro-negative substances, such as copper pyrites, iron pyrites, &c., it would be decomposed, and enter into combination with oxygen, &c.

I might extend my remarks to other points; but I refrain, not having leisure for such employment.

XVIII. *Electro-Magnetic Telegraph. By* PROFESSOR MORSE, *of the New York City University.*†

While a contest is waging in several countries of Europe—in England, Scotland, France, and Germany, for the discovery and invention of the Electric Telegraph, it may not be amiss to state, that America also claims to be an independent discoverer and claimant for priority in the invention. The dates, the names of the inventors, and other circumstances will doubtless ere long be published, and then the world can judge between the conflicting parties. In the mean time it is well ascertained, that Professor Morse conceived and planned, five years ago, an electric telegraph, while on his passage home from France, and immediately on his landing, he commenced the machinery. Early last spring, *in April*, the general features of his plan were very extensively published in the newspapers, and very lately, *in August*, we learn that several telegraphs on the basis of electricity are in various stages of progress in Europe.

The distinguishing features of Professor Morse's telegraph are a *register*, which permanently records in characters easily legible the fullest communication, and the use of but *one wire* as a conductor; although for greater convenience of communicating at all times, and of having a whole circuit at command from each extremity of the line, he will use four wires.

On September 2d, Professor Morse tried an experiment with a circuit of copper wire one thousand seven hundred feet in length, and of the minimum size of No. 18 wire. The record of the register was sufficiently perfect to demonstrate the practicability of the plan. On the 4th of September some slight changes were made in the machinery, when the register recorded perfectly the signs exhibited in figs. 23 and 24, Plate IV.

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* Annals of Electricity, vol. I. p. 227.

† Silliman's American Journal for October, 1837.

The *words* in the figures were the intelligence transmitted.

The *numbers*, (in this instance arbitrary,) are the numbers of the words in the telegraphic dictionary.

The *points* are the markings of the register, each point being marked every time the electric fluid passes.

The register marks but one kind of mark, to wit, (V.) This can be varied two ways. By intervals thus (V VV VVV) signifying one, two, three, &c., and by reversing thus (Λ); examples of both these varieties are seen in fig. 24.

The single numbers are separated by *short*, and the whole numbers by *long intervals.*

To illustrate by the diagram, the word, "successful" is first found in the dictionary, and its telegraphic number 214 is set up in a species of type prepared for that purpose, and so of the other words. The types then operate upon the machinery and serve to regulate the times and intervals of the passages of electricity. Each passage of the fluid causes a pencil at the extremity of the wire to mark the points as in the figures.

To read the marks; count the points at the bottom of each line. It will be perceived that two points come first, separated by a *short* interval from the next point. Set 2 beneath it. Then comes one point likewise separated by a *short* interval. Set 1 beneath it. Then come four points. Set 4 beneath it. But the interval in this case is a *long* interval, consequently the three numbers comprise the whole number 214.

So proceed with the rest until the numbers are all set down. Then by referring to the telegraphic dictionary, the words corresponding to the numbers are found, and the communication read. Thus it will be seen that by means of the changes upon *ten* characters, all words can be transmitted. But there are *two points* reversed in the lower line. These are the *eleventh* character, placed before a number to signify that it is to be read as a *number*, and not as the representative of a word.

Since the 4th of September, one thousand feet more of wire No. 23 have been added, making in all two thousand seven hundred feet—more than half a mile of a reduced size of wire: the register still recorded accurately.

Arrangements have been made for establishing a circuit of several miles, and for constructing new and accurate machinery. Professor Gale, of the New York City University, is engaged with Professor Morse in making some interesting experiments connected with this invention, and to test the effect of length of wire on the magnetizing influence of voltaic electricity.

XIX. *On Zinc, as a covering for buildings: in a letter from Professor* A. Caswell, *to Messrs. Crocker, Brothers, and Co.*[*]

You some time ago requested me to examine an article on zinc, *as a roofing material,* published by Dr Gale, of New York, in a late number of the Mechanics' Magazine. I regret that it has not been in my power to give your request earlier attention.

The remarks of Dr. G. which were copied by several papers at the time, were fitted, in your opinion, to prejudice the public mind unjustly upon a subject of great importance. He discourages the use of zinc as a roofing material, upon several distinct accounts, the principal of which are the following:

1. The difficulty of making the roof tight.
2. The deterioration of the water which falls from it.
3. The comparatively small resistance which it offers to the progress of fire.

1. As to the first of these objections, the brittleness of the metal and its great expansion from heat are adduced, to show that a roof cannot be made sufficiently tight. Zinc in the unwrought state is well known to be very brittle, and there may be in the market *rolled* or *sheet* zinc of a bad quality. But no one need be deceived on this point, since nothing is easier than to test its flexibility. Sheet zinc which will bear to be doubled and hammered down without any appearance of fracture in the bend, may be used as a covering for buildings, without the least fear of leakage. Such is the fact with regard to sheet zinc which I have examined from your manufactory: and such, I am assured, is the fact with regard to foreign zinc from the best manufactories. But any detailed examination of the brittleness and expansion of zinc, so far as the question is concerned, is entirely obviated by the well ascertained fact, that there is no practical difficulty in making a zinc roof *perfectly tight.* The numerous certificates which you have submitted to my examination, from most respectable gentlemen, who have made the experiment, place the subject beyond all reasonable doubt. A zinc roof may be as easily made tight as any other whatever.

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[*] From Silliman's American Philosophical Journal.

2. The second objection respects the deterioration of the water which falls from the roof. This consideration is particularly important to all those who are in the habit of using cistern water for culinary and other purposes.

It is alleged that a *poisonous* sub-oxide of zinc is dissolved in the water, which renders it unfit for *cooking*, and impairs its properties for *washing*. On this point I have consulted the ablest modern writers on chemistry—Brand, Turner, Thomson, Berzelius, and others. The oxides of zinc seem not to have been much studied. The principal one known, and perhaps the only one certainly known, is the white oxide, (sometimes called the flowers of zinc,) which is quite insoluble in water, and hence could not vitiate it properties. Berzelius thinks there are two others.

The *sub-oxide* is the grey coating formed on the surface of zinc by exposure to the weather, and this is the substance which, it is said, is dissolved and mixed with the water, which falls from a zinc roof, thereby impregnating it with deleterious properties. This opinion, so far as I can learn, is unsupported by any writer on chemistry. Turner says, "zinc undergoes little change by the action of air and moisture." Aikin's chemical dictionary, a work of merit and authority, says, "the action of air upon zinc, at the common temperature, is very slight; it acquires a very thin superficial coating of grey oxide, which adheres to the metal and *prevents any further change.*" The statement of Thomson is, that zinc, when exposed to the air, soon loses its lustre, but "*scarcely undergoes any other change.*" The account given by Berzelius, the ablest chemist of the age, is very explicit and much to the point. He says, "this oxide is formed on the surface of zinc which remains a long time exposed to the air. It has a dark grey colour when moistened, but by drying, becomes of a light grey. Ordinarily it forms a thin crust, on the surface, which neither *increases* nor *experiences any change in the air:* but acquires great hardness, and resists, better than the metal itself, the mechanical and chemical action of other bodies. A piece of zinc sufficiently sub-oxidized, at the surface, dissolves with *extreme slowness in the acids*, and only at the boiling temperature."

Such are the opinions of chemists, and particularly of Berzelius, whose unrivalled skill and accuracy in chemical analysis, have been the admiration of all contemporary chemists.

The opinion of Dr. G. is considerably at variance with those now adduced. I think he has not stated very fully, and certainly not very satisfactorily, the reasons on which it is founded. He mentions, however, as a proof that this sub-oxide is dissolved in water from zinc roofs, that if it is suffered to stand for some time exposed to the air, the sub-oxide gradually takes oxygen from the atmosphere, and is thus converted into the *insoluble* white oxide before mentioned, and is then precipitated in the form of a white powder. To test its purity by this method, I have kept water from a zinc roof exposed in clean glass vessels for several days, without any, the slighest appearance of a precipitate, or even a pellicle upon the surface. And what is still better as a test, I have kept it for several days in closed bottles with oxygen gas, and subjected it to frequent agitation, without the least appearance of a precipitate, or any diminution of transparency. I must think, therefore, that if such water contains the sub-oxide of zinc, its presence is not to be detected in this way.

That the quantity of zinc dissolved in water *must be exceedingly small,* is obvious from the following consideration. A sheet not more than the fortieth of an inch in thickness, would probably last at least half a century, on the roof of a building. Indeed, for any thing that we know as to the *rate* of its oxidation, it might last for centuries. The current opinion of chemists, and this confirmed by observation and experiment, so far as these have extended, is, that after the grey oxide is once formed, any further change takes place *scarcely at all,* or with *extreme slowness.* But on the supposition that it would last only fifty years, the whole quantity of rain which falls in the course of a year, or about three feet on the level, would dissolve the *two thousandth part* of an inch in thickness of zinc. This, to produce any appreciable effect, must be one of the most virulent poisons, at least equal to prussic acid. But so far from being an active poison, it remains to be shown that it is poisonous at all, even if a minute portion of it did mingle with the water. The white oxide of zinc is not poisonous, and the inference seems to be gratuitous that this is so.

It is due no less to the public than yourselves, that the truth upon this subject should be known and promulgated. I am quite satisfied, for one, that we are not in the least danger of being poisoned by the use of water from zinc roofs. The portions of this water which I have examined, could not be distinguished from pure river water by any test that I have been able to apply to it. I feel myself warranted, therefore, in the conclusion, that *it has suffered no deterioration whatever from zinc.*

3. A third objection is that zinc affords inadequate protection against fire.

This objection is based upon the fact that zinc melts at a low temperature; and in case of fusion leaves the wood-work of the building unprotected. This objection is rather specious than real. Zinc melts about the temperature of 700 Fahr. or a little below red heat. Whenever, therefore, the heat from adjacent buildings is anything less than that of redness, zinc would afford as complete protection as copper or iron. When the heat has reached the melting point of zinc, which it would seldom do except in the most compact parts of cities, very little confidence could be placed in iron or copper. The dry wood work of the roof, under a covering of red hot iron, with air enough for combustion circulating through openings and crevices, would soon be in flames: and when once in flames it would be extremely difficult to extinguish it by the application of water. It would be applied with great disadvantage to the under side of the roof, and almost to no purpose at all upon the top. If therefore the heat, in any case, should become so intense as to melt zinc, the probability of protection from iron or copper will be but small.

Complete protection against fire, is perhaps, unattainable; at least we can never be sure that we have attained it. In the progress of the arts, great improvements, no doubt, will be made in the mode of defence against the attacks of this great destroyer. I am not aware that the following construction for a roof has ever been tried. For cheapness, tightness, durability, and resistance to fire, it seems to be well deserving the attention of builders. Let the rough boards of the roof (and the rougher the better) be covered with a thick coating of common lime mortar, then lay down the *ribs*, if I may so call them, for the zinc plates, then cover the whole with zinc, according to the most approved method of applying it. Such a roof would be in no danger of leakage, unless the water accumulated upon it so as to stand above the ribs, in which case no roof would be tight unless it were caulked or soldered throughout. This covering, if I am rightly informed, would be nearly as cheap as slate, quite as cheap as tin, cheaper than iron, and more than three times cheaper than copper: and would at the same time resist fire better than any of them. A heat that would melt down the copper and iron, would, of course, melt the zinc, but would leave the mortar uninjured. The peculiar advantage of the mortar is, that it is infusible except at a very high temperature, while the closeness with which it adheres to the wood-work, is such as to exclude the air and thus prevent combustion. If the mortar should be kept at a red heat for some length of time, the wood beneath it would be *charred*, but could hardly be *burnt*. In case of fusion, the zinc might be replaced without injury to the mortar. I know of no construction for a roof that would be more completely fire proof than this.

Such are my views on the subject to which you called my attention. If they shall serve in any measure, to remove prejudice, and allay unfounded apprehensions on a subject of great and growing importance to the public, it will afford me much pleasure.

XX. *Description of an electro-magnetic engine, by* Mr. J. P. Joule.

Sir,

I am now making an electro-magnetic engine; and as I imagine that I have succeeded in effecting considerable improvement in the construction of the magnets, and the whole arrangement of the instrument, I hope you will allow me to lay it before the numerous readers of your valuable Annals.

In fig. 19, Plate III., *e, f,* represents a side view, and *b, b,* the poles of the magnets which I propose. I think it best to have, *in all cases,* the distance *a,* between the poles about the fifth part of an inch, and the thickness of each pole or arm of the magnet, the same; or, perhaps, rather less (if the magnets are required to be of greater power, the breadth *cd, cd,* or length *e f,* should be increased). Covered wire should be wrapped round them, until the interstice between the arms of the magnets is completely filled.

The advantages obtained by using magnets of this description may be seen on inspecting fig. 20: the small arrows indicate the direction that the electricity takes in passing from P to N.

1. The wires round each arm are kept close to the substance of the iron, on account of its small thickness.

2. The electricity going in the same direction between the arms, it is obvious that the greater part of each arm receives the magnetizing effect of a double quantity of wire.

3. A great saving of room is effected, and, consequently, the power relative to the weight of the engine is increased.

4. That objection is obviated which applies to those arrangements, in which the poles are at all distant from each other: viz. that the magnets during a great part of their rotation are almost inactive.

I have made several of the magnets of the above description. Their lifting power is very good: the spark on breaking battery contact is remarkably brilliant, and, consequently, rather disadvantageous.

The magnets thus constructed are then arranged into two compact circles, one of which is represented in fig. 21, *a, e, f,* attached to the board *a, b, c,* the wires of each magnet communicating with those of its neighbours, so that the whole may be magnetized at once by means of the wires, *h*, *i*. The whole of the moveable circle supposed to be immediately above fig. 21, and seen at *m* fig. 22, is magnetized and reversed, by the commutator *g*, *g*, *g*. It consists of two pieces of bright sheet brass (in the figure the black and white cogged circles are meant to show their different polarity,) inlaid in the wood, and communicating with the opposite poles of the battery by the wires *k*, *l*; the wires *n*, *o*, proceeding from the moveable system represented in fig. 22, (where the stationary magnets are omitted to avoid confusion to the axis A, B,) communicate at the clamp *p*, which secures them steadily to two steel springs which press gently on the commutator, and allow by their flexibility of the reversion of the motion.

c, *q*, *r*, *g*, is iron work fastened to the board *a*, *b*, *c*; to the axle A, B, wheels or paddles may be affixed so as to answer either locomotive or sailing purposes. It will be readily seen that by a proper adjustment of wires to the cups, *h*, *i*, *k*, *l*, the electricity may go at once through both the fixed and moveable systems of magnets, or, that a distinct current may traverse each.

The particular machine which I am making will consist of 40 magnets, each two inches long, and 3-eighths of an inch broad, the rest of the dimensions as stated before; the whole size of the machine will not be more than a six inch cube. When this is completed, I hope to give you a particular account of its duty.

I am, yours truly,
J. P. JOULE.

Salford,
January 8, 1838.

XXI. *On a revolving Electro-Magnetic Instrument. By* DR. BENJAMIN RUSH MC.CONNELL.*

In a letter to Professor Silliman.

Mauch, Chunk, Pennsylvania,
June 25, 1837.

Sir,

Up to this date, I had entertained the hope of having ready for the ensuing number of the "Journal of Science," a digested series of electro-magnetic experiments, the results of some enquiry involving something of novelty at least, if not of much interest. The pressure of professional duty, however, as colliery surgeon, and the physical character of my district, (which you are personally familiar with) have hitherto prevented me. I now take advantage of a leisure hour, which after all may be too late for your next number, to place on record the subjoined facts, about which I confess I feel anxious. I have—and have had for nearly a twelvemonth—in operation, an *electro-magnetic engine*, of a construction and upon a principle essentially different from anything hitherto announced. In the course of a series of galvanic experiments, which for several years past have assisted to beguile the intervals of professional labour, my attention was drawn to the mutual action of *rectilineal* and *circular* currents, as a highly promising source of motive agency for practical purposes. After innumerable failures, I eventually succeeded in constructing a machine which may be fairly pronounced *perfect* upon the *actual scale* of its construction; its value upon a *working scale* remains to be proved. The general arrangement of my machine is not unlike the philosophical toy, invented, I believe, by Mr. Sturgeon,* of London, as long since as 1828 or 1829, of two copper discs, one at either end of a common shaft, revolving each in its own trough of mercury, between the poles of two horse-shoe magnets. Such is my machine in general, with the addition of a band or cogwheel on the centre of the same shaft, intermediate between the discs, which revolve between the poles of electro-magnets, without the intervention of a fluid medium of any kind as a part of the circuit; the mode of accomplishing this constitutes the peculiarity of its claim, and you, sir, are competent to appreciate its novelty and value. My electro-magnets are *hollow* (another new feature), and have been made with nearly equivalent results of bar iron, of tinned iron, and of copper.

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* From Silliman's Journal, for October, 1837.

The magnets I now have in use are one inch in diameter, one inch and three quarters between the poles, and five inches and a half in length, each wrapped with one hundred and fifty feet of iron (bonnet) wire. My battery is rectangular, and consists of two concentric boxes of sheet copper, with a zinc box included, the whole constituting a square box open in the middle. The exterior box is seven inches square, the zinc seven deep by six and a half in each of its other dimensions, the interior box of copper seven by six: the whole will contain something more than a quart of the acid menstruum, and presents about two-fifths of galvanic surface in the aggregate. The driving or wheel-band is sixteen inches in diameter (the discs being nine inches each), the shaft of iron three-eighths of an inch thick and five inches long, working in brass bearings. The battery is charged through a cock in the platform, (which is of cherry, one inch thick by twelve square, and is attached to the battery by seven bolts,) and discharged when necessary, to cleanse or replenish, by a cock near the bottom. The whole swung in *gimbals* between two turned posts, communicates motion from the band through the platform and centre of the battery to the propelling wheel, on the axle of a small carriage. The driving wheel revolves when not loaded, (sixteen inches in diameter) about two hundred times in a minute, traversing upwards of eight hundred feet! and will revolve *seventy times* per minute, carrying a load of forty pounds, through a space of two hundred and eighty feet, in that time, being a performance nearly equal to the power of three men. The entire machine occupies a space of two feet in height by a foot in breadth, and weighs, when charged for service, seventeen pounds. Touching a small *lever* reverses the action of the engine instantaneously; raising another *arrests* its action, or, technically, throws it out of gear. Thus, sir, as you will perceive, my engine differs totally from the ingenious arrangement of Messrs. Davenport and Cook, of whose galvanic machine your April number contained a notice. Justice to myself, without intending to depreciate in any degree the labours of others, requires from me the statement that my engine, *as it stands*, substantially, was in existence *nearly two years since*, the greater part having been made to my order by the mechanics of this village, in the summer of 1835; an improved portion of the moving parts (of finished workmanship) was made in Philadelphia, as long since as January of the present year, by Mr. J. Mason, Philosophical Instrument Maker, of Green-leaf Court. I may also state, that Professor Hare, Mr. Isaiah Lukins, Mr. S. V. Merrick, Mr. E. Hazard, and other scientific and personal friends whom I met in society on occasion of a visit to that city at the period above referred to, were cognizant of my experiments and objects many months previous to the public announcement of results of a similar kind from *any other* experimenter. The field of investigation is large, and as yet not much explored, and Mr. Davenport may rest assured, that should this notice chance to meet his eye, that no one will rejoice more sincerely than I shall to hear of his onward progress, while I myself hope also to advance in the march of improvement.

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* Mr. Barlow made the first experiment with wheels, which were cut into a star-like form. Edit.

Remarks on the preceding paper.

We have heard of a great variety of engines, each of which has some peculiarity that claims attention: and the one just now noticed, (not described) is certainly very different to any we have ever before seen announced. We however know of a *trial* made some years since, to obtain power for an engine by a similar application of electro-magnetism, by a fellow-labourer on this side of the Atlantic; and the result answered our predictions: it was a complete failure. We are well aware of the extent of power that is likely ever to be gained by the revolving wheels between the poles of magnets, and could never have expected to have heard of an engine, founded on that principle, which possessed the extraordinary advantages which Dr. Mc.Connell has applied to his. We think it is much to be lamented that Dr. Mc.Connell did not send the figure of his engine to Professor Silliman as was requested. Correct drawings of instruments frequently convey more intelligence than the most elaborate description in any other shape. We should have had much pleasure in copying it for the benefit of our home readers.

We think ourselves particularly fortunate in having announced our hollow iron magnets before Dr. Mc.Connell's was heard of in this country. When that gentleman reads No. 6 of these "Annals," which was published on the *same day* as the Journal in which his paper appears, he will find, that hollow magnets of iron were used in 1830. But we must confess that we are very far behind him in the production of *copper* magnets. EDIT.

XXII. *On the Conduction of Water. By* PROFESSOR C. DEWEY.*

In vol. XXVIII. page 151, of this Journal (Silliman's Journal) are some details on this subject. In that paper, the inadequacy of Dr. Murray's experiments on this subject was shown. It is certain that when the vessel containing the water and thermometer is formed of *ice*, the power of water to conduct caloric downwards *cannot be shown*, as the heated water, when its temperature is below 40° Fahr. will become heavier, and thence sink to the bulb, and cause the temperature to be higher. If the vessel is not made of ice, and the water on the thermometer is cooled to near 32° Fahr. it will be equally impossible to show its conduction of caloric from particle to particle, for the very same reason as before, unless the caloric is applied at the bottom. The experiments detailed in that paper were conclusive on the conduction of water when its temperature was above 40° Fahr. unless the caloric was chiefly conveyed to the water along the sides and bottom of the vessel used. In some late experiments on this subject, this difficulty has been removed, and the possibility prevented by the following contrivance. A thermometer was immersed in water at 62° Fahr., so as to be 3-eighths of an inch deep on the bulb, in a large earthen dish. A hollow glass cylinder, four inches in diameter and two inches high, was then placed in the water so as to have the bulb of the thermometer in the middle of the cylinder. The cylinder was prevented from touching the bottom of the dish by three small pieces of wood placed under it. The ether, which was to be inflamed over the bulb, was thus confined within the hollow glass cylinder, so that the generated caloric could not come to the sides of the earthen dish. When heated oil was poured over the bulb, it was confined in the same way. The influence of a heated iron was confined in the same manner. Yet when all these were repeatedly tried, the temperature rose about *six* degrees, except that the iron did not heat it so much. These experiments satisfactorily prove that *caloric passed downwards.* If it was not *radiation,* it must prove the *conduction of water.* The form of the experiment prevents the heating of the bulb by means of the dish. It was clear that the rise of the thermometer soon ceased, as it ought to do if it were conduction; for the heated particles, being made lighter, would be pressed upwards by the cool and heavier particles around them as soon as the conduction was much diminished by the cooling above. Hence after a few moments the thermometer would begin to fall, although the surface of the water was several degrees above that part of the water in contact with the bulb. When an air thermometer, with its stem passing down through the neck of a funnel of glass, and made tight in the neck by a cork, is immersed to the depth of an inch in water over its bulb, and then the hollow cylinder of glass is made to surround the bulb, as in the other case, and kept from touching the funnel, and ether is inflamed within the cylinder, the experiment is clearly visible, beautiful, and decisive. It is not obvious how any experiment can be more satisfactory than this: any experiment of its character—for we must always except the common one of mixing heated and cold water, when the caloric must *pass from particle to particle,* as the temperature of the hotter is instantly diminished, and of the colder is instantly increased.

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* From Silliman's American Philosophical Journal.

XXIII. *An account of a Hurricane, which visited Shelbyville, Tennessee, June 1st,* 1830; *communicated to the Connecticut Academy. By* DR. J. H. KAIN.

Few occurrences give us such awful conceptions of the power of the unrestrained elements, as the agitations of our atmosphere. Accounts of storms at sea are common, and to those who make the great waters their home, they are every day occurrences. But, happily for the human family, such hurricanes as that which visited Shelbyville in 1830, are rare. The ocean is easily agitated and thrown into violent commotion; but it requires a much more powerful wind to disturb the repose of those solid bodies which the earth's gravity has bound to her bosom. The effects of a storm at sea are much less dreadful and terrific than the devastations of a land hurricane. A fine ship may safely weather the most violent gale at sea; but probably no building or work of man could encounter, without instant destruction, the fury of the hurricane when it meets with the unyielding resistance of the solid land. Not only are many buildings torn to pieces and scattered about in astonishing confusion; but the largest trees are twisted off at the trunk and hurled aloft like pieces of paper in an ordinary breeze.

Some countries appear to be more subject to tornadoes than others. This is a well known feature of the climate of the West India Islands. Numerous vestiges of hurricanes are seen in Tennessee. In some places you may trace for thirty miles the track of a tornado, which has prostrated the forest in its course, and piled up its ruins in large masses; sometimes they appear quite recent, and nature has not repaired the waste; the splintered stumps still standing, the bark still covers the prostrate trunk, the branches and tops of trees are still intertwined, and perhaps even the brown and decaying verdure of the leaves presents the appearance of a premature autumn; the roads are stopped up and impassable; fences and farm houses have disappeared; the corn, wheat, and cotton lie flat upon the ground, as if a roller had passed over them; in some places large piles of drift are seen heaped against a hill or rock, and the mud has settled upon and buried the vegetable productions of the earth. At other times you see merely the vestiges of an old hurricane. A new growth has sprung up in the woods, and you may remark the uniform size of the young trees, all dating their age from the same epoch. The bark has decayed away, and the large trunks of the fallen trees are covered with moss; their limbs and tops have rotted and disappeared, and the roots are still distinguished by the mass of earth which was torn up with them, and is now settling down, and still by the uniformity of their position mark the exact course of the hurricane, the root always being towards the point of the compass from which the wind blew. In other places we may find the vestiges of still more ancient date. The process of decay has been completed: even the trunks of the fallen forest have disappeared, a tall, rich, and luxuriant growth has again overspread the earth, and we can only read the history of former devastations in the numerous hillocks of yellow, upturned earth, left by the roots of trees, which, after being blown down, have entirely disappeared and mingled with the rich, black soil in which they had grown. The tracks of these hurricanes are not often more than one hundred rods wide, and vary from a mile to twenty or thirty in length. You can never tell from the direction in which the trees have fallen, the general course of the hurricane. This is usually from southwest to northeast, but though the trees at any particular spot lie parallel to each other, their direction varies very much at different places of the same track. At one place they have fallen with their tops to the north, at another they have fallen towards the south, and at another to the east or west. This fact strengthens the theory of Mr. Redfield, which ascribes to winds, storms, and tempests a gyral form.

It will be remembered by those who have read Mr. Redfield's very ingenious essays, that he suggests the theory that the storms which visit our coast rise on the gulph of Mexico, and assuming a gyral motion, sweep over the United States from the southwest to the northeast. It is known to all who have resided in the great Valley of the Mississippee that there is a constant current of air setting in from the Gulph and blowing up our water courses. This is occasionally interrupted for a few days by a wind in a contrary direction, accompanied usually by rain. Probably this is only an apparent variation produced by the gyral motion of the wind operating on a very large and extended scale. The smaller gyrations which produce our thunder gusts and tornadoes come very sensibly from the southwest. It will be seen from an inspection of the map of North America, that the mountains of Tennessee present the first obstruction which this great southwestern current of air meets with in its progress across our continent. That country is in a position which, while it catches and is refreshed by the softest zephyrs and the most refreshing showers of this great atmospheric current, likewise exposes it to the first rude blasts of its angry tempests. More than any other portion of the United States it bears on its bosom the scars of many an awful contest with this tremendous power, and its uprooted forests tell us too plainly the overwhelming force of the unconquered enemy.

The writer had an opportunity of witnessing one of those awfully grand and terrific convulsions of the atmosphere, which nearly destroyed the town of Shelbyville, in the month of June, 1830. For some days previous to the catastrophe the air had been unusually calm, sultry, and oppressive. It was a very fortunate circumstance for the inhabitants of the village that they were reposing quietly in their beds when the tornado swept over them. Had it occurred in the day time, when the people were moving about, and when the doors and windows were all open, the loss of life must have been much greater. It may be well to remark here, that it was found from the experience of that night, that the complete closing of doors and windows, so as to exclude the external atmosphere, was of the utmost importance. Not a house stood, whose doors and windows were left open, or were too weak to resist the impulse of the wind. On the night of the storm at Shelbyville, a strong western gale blew throughout the State of Tennessee, and several distinct gyrations were formed in different portions of the current. The town of Charlotte, sixty miles northwest from Sherbyville, was blown down two hours before the destruction of the latter. Another gyration took place twenty miles northeast of Shelbyville, which destroyed a farm, and was equally violent with that at Shelbyville. The clouds began to cluster in the west, and the wind to blow, at an early hour of the night, but the storm did not reach its utmost fury till midnight. The lightning was unusually brilliant, the flashes were so continuous as to enable us to see objects with perfect distinctness, and even to read without the light of a candle. This unusual brilliancy of the lightning was remarked in many distant parts of the State. The lightning was not accompanied with very loud thunder, nor did it appear to have struck or injured any object in the neighbourhood of the village.

The town of Shelbyville is situated on a hill which fills up, so to speak, a long gorge between two chains of highlands, which lie on each side of Duck River; this hill is at the eastern extremity of the valley. This circumstance contributes very much to the pleasantness of the site for a town, commanding a fine view, and catching every breeze of summer; but it likewise exposes it to the fury of every gale that sweeps up the river. The court-house occupies the brow of the hill. Around the court-house is a small square or common, and on the four sides of this square are built the principal stores and shops, the bank, and the taverns. It was on this part of the town that the hurricane exerted its greatest violence. Few families resided in this portion of the village; and it was mercifully ordered that the catastrophe should occur at an hour when the inhabitants had retired from the business part of the town. The wind had blown with great fury and violence without doing any injury for three hours, when suddenly the houses began to crack, and in fifteen seconds the besom of destruction swept over the devoted village, and left it a mass of ruins. Those who were within the range of the tempest were warned of their danger by the shaking of their houses the moment before they fell. A change of position saved the lives of some, and caused the death of others. Some found themselves suddenly in the open air, surrounded by falling timbers, planks, and bricks; others were buried in the ruins of their houses. Some met death whilst endeavouring to escape; others perished in their beds, crushed beneath their falling dwellings. Only five persons were killed. A few were dreadfully bruised, who recovered from their wounds. The interpositions of a merciful Providence for the preservation of life in the midst of such danger, were numerous and astonishing to all who knew the facts, and so much out of the way of common events, that they would scarcely be believed on the testimony of a single individual. Whole families were rescued from the ruins of their houses, without any material bodily injury. Individuals were blown about in the air like feathers and escaped without a scratch. A young lady of uncommon courage and presence of mind, who was out of doors, and had escaped from a falling house, described the scene as awfully grand and magnificent. "I looked," said she, "for well known places, and they had vanished. I turned to go into the house and it was gone. I went for the kitchen, it likewise was not to be found: I looked up and beheld the lightning flashing vividly upon floating planks, plaister, bricks, and shingles, all glistening like white pieces of paper, and filling the air; around me I beheld the white walls of uncovered houses glaring in the light of the storm."

The court-house, a fine brick building, was blown down to the ground. The bank, several taverns, a church, and many stores, shops, and dwelling houses were laid in ruins. Some houses were merely unroofed: others were blown away to the first story, and others were laid prostrate with the ground. The gyral form of the tempest was evinced by the manner in which the materials of the same building were scattered about in different directions, and by the testimony of an individual who declared that he was carried around in a circle of fifty yards diameter with a piece of timber to which he held fast, and by which he was dreadfully bruised. He was picked up two hundred yards from his bed fellow, and in a contrary direction from the house in which they slept. The general direction of the wind was from west to east. The gale was succeeded by rain in quantities almost amounting in devastating power to an avalanche. No hail fell during or subsequently to the storm. The following facts will show the velocity and force of the wind. Laths were shot into brick walls with such force as to penetrate between the bricks, and then turn up and break off, and laths were seen to have penetrated through the boards of houses. Heavy pieces of timber were carried to great heights, and falling penetrated the roofs of houses. Pieces of plank and shingles were carried along the path of the storm, and strewed on the ground for three miles from the town. A book belonging to an attorney, whose office was blown away, was found seven miles from the village. Doors were blown from their hinges, locks and bolts were forced, and if a wall proved strong enough to resist the violence of the wind, large masses of ruins were found piled up against it.

A remarkable phenomenon, which was confirmed by the observation and testimony of many witnesses, was, that sound was not conveyed by the atmosphere to any distance, owing perhaps to its velocity. Not one individual without the range of the hurricane, heard the fall of the houses. The overturning of such a number of houses, in a calm time, would have produced a very loud sound. Still louder would be the sound of so many substances torn asunder, crushed and broken, and dashed to pieces. But no sound whatever was heard by those without the storm, if we except the shrill whistling of the wind, like a loud bugle high in the air. Those who were within one hundred feet of the falling houses did not hear them fall. Nay, we did not hear the fall of the trees, which were torn to pieces and piled around our house. We were not even aware of our danger. Within doors we conversed, and were heard in the ordinary tones; but we were unconscious of what was going on without, until informed by the arrival of fugitives from the awful scene. It was remarked, too, by persons in the falling houses, "we heard nothing but the crash of our own house."

Another fact, which it is important to recollect, is, that it was observed that the corner of the house, on the first floor, next the wind, was the safest part of the building. In a brick house, the cellar was a very unsafe place, because if the joists gave way the cellar was filled with the materials of the building.

The side of the house opposite the wind was very unsafe, because the materials of the building were blown to that side. A small portion of the wall next the wind always stood. Brick houses were less safe than framed houses. They were more liable to be blown down, and their materials were more dangerous. A young man saved his life by creeping under a bench, which afterwards sustained a mass of many tons. Some were preserved by getting under their bedsteads. No place in the upper story of a house was safe. The recollection of these facts may be useful to us, should we be so unhappy as to be exposed to a similar catastrophe, though unfortunately at such a time we are not apt to recollect any thing, and are too liable to be deserted by our reason and presence of mind.

P. S. An intelligent farmer, who lived on the highlands, eight miles south of Shelbyville, in a situation which commands a view of the hill on which that village is built, communicated to the writer a fact which is curious, and may throw some light upon the nature of the forces which produce the gyrations of hurricanes. He had risen about midnight to look out on the storm, his attention having been excited by the unusual brilliancy of the lightning, and the continuousness of its flashes. The heavens were overspread with dark clouds in rapid motion. There was a strong western gale. The lightning appeared to issue from a cloud which was moving very swiftly towards Shelbyville. This cloud was permanently luminous, and between the flashes of lightning of the color of red hot iron. In shape it was double, and the two portions approached each other like the wings of an eagle, and on passing over the village, the wings suddenly coalesced and descended, and then became invisible to the observer. This occurred, as nearly as we could calculate, at the moment when the hurricane swept over the town.

It has been suggested to me by a friend, that at the moment of the union of the two clouds, two contrary currents of air met, and produced the whirlwind, which was so destructive in its effects.

XXIV. *On the Meteoric Shower, of November,* 1836. *By* DENISON OLMSTED, *Professor of Natural Philosophy and Astronomy in Yale College.*

For six years in succession, there has been observed, on or about the 13th of November of each year, a remarkable exhibition of *shooting stars,* which has received the name of the "Meteoric Shower."

In 1831, the phenomenon was observed in the State of Ohio,* and in the Mediterranean, off the coast of Spain.† In 1832, the shower appeared in a more imposing form, and was seen at Mocha, in Arabia;‡ in the middle of the Atlantic Ocean;§ near Orenberg, in Russia;‖ and at Pernambuco, in South America.¶ The magnificent Meteoric Shower of 1833, is too well known to require the recital of any particulars. Of the recurrence of the phenomenon at the corresponding period in 1834, and in 1835, evidence has been presented to the public in previous numbers of this Journal. (See Vols. xxvii, pp. 339 and 417. xxix, 168.)

I now feel authorized to assert, that *the Meteoric Shower re-appeared on the morning of the* 13*th November,* 1836.

It has been supposed by some, that the appearance of an extraordinary number of shooting stars, at the several anniversaries since the great phenomenon of November, 1833, can be accounted for by the fact, that so general an expectation of such an event has been excited, and that so many persons have been on the watch for it. Having, however, been much in the habit of observing phenomena of this kind, I can truly say, that those exhibitions of shooting stars which have for several years occurred on the 13th or 14th of November, are characterized by several peculiarities which clearly distinguish them from ordinary shooting stars. Such peculiarities are the following.

1. The *number of meteors,* though exceedingly variable, is much greater than usual, especially of the larger and brighter kinds.
2. An uncommonly large proportion leave *luminous trains.*
3. The meteors, with few exceptions, all appear *to proceed from a common centre,* the position of which has been uniformly in nearly the same point in the heavens, viz. in some part of the constellation Leo.
4. The principal exhibition has at all times, and at all places, occurred between midnight and sunrise, and *the maximum from three to four o'clock.*

In all these particulars, the Meteoric Showers of 1834, 5, and 6, have resembled that of 1833; while no person, so far as I have heard, has observed the same combination of circumstances on any other occasion within the same period. I have not supposed it necessary, in order to establish the identity of these later meteoric showers with that of 1833, that they should be of the same magnitude with that. A small eclipse I have considered a phenomenon of the same kind with a large one; and, conformably to this analogy, I have regarded an eclipse of the sun, first exhibiting itself as a slight indentation of the solar limb, but increasing in magnitude at every recurrence, until it becomes total, and afterwards, at each return, but partially covering the solar disc, until the moon passes quite clear of the sun,—as affording no bad illustration of what probably takes place in regard to these meteoric showers. The fact that the Aurora Borealis appears unusually frequent and magnificent for a few successive years, and then for a long time is scarcely seen at all, was proved by Marian, a hundred years ago.* There is much reason to suspect a like periodical character in the phenomenon in question, which first arrested attention in 1831, became more remarkable in 1832, arrived at its maximum in 1833, and has since grown less and less at each annual return. Some seem to suppose that we are now warranted in expecting a similar exhibition of meteors on the morning of every future anniversary; but this, I think, is not to be expected. It is, perhaps, more probable, that its recurrence, unless in a very diminished degree, will scarcely be witnessed again by the present generation. The shower, however, at its late return, was more striking than I anticipated; and it must be acknowledged to be adventurous, to enter the region of prediction respecting the future exhibitions of a phenomenon, both whose origin and whose laws we so imperfectly understand.

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* American Journal of Science, xxviii, 419.

† Bibliothèque Universelle, Sept. 1835.

‡ Amer. Jour. xxvi, 136. § Ibid. 349.

‖ Ed. New. Phil. Jour. July, 1836.

¶ New York American, Nov. 15, 1836.

But it is time to present the reader with the evidence of the return of the meteoric shower on the late anniversary.

Accounts of observations before us show, that the meteoric shower was seen in most of the Atlantic States from Maine to South Carolina. We will begin on the north.

I. Observations made at SPRINGVALE, MAINE. *Extract of a letter from Samuel Dunster, Esq., Agent of the Franklin Manufacturing Company.*

"I requested the watchman at our manufacturing establishment to call me, if anything of interest occurred. He accordingly called me at about a quarter before three o'clock,

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* Traité Phys. et Hist. de L'Aurore Boréale. Par M. De Marian.—Memoirs of the Royal Academy of Sciences for 1731.

(on the morning of Nov. 13th.) At three o'clock I began to count the meteors, and numbered as follows.

Time.	Number.
3 h. 30 m.	37
3 h. 45 m.	25
4 h.	31
4 h. 15 m.	25
4 h. 30 m.	22
4 h. 45 m.	28
5 h.	22
5 h. 15 m.	16
5 h. 30 m.	20
5 h. 45 m.	11
6 h.	11
6 h. 15 m.	5
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"The meteors, with the exception of five or six, all had a direction from a point in the eastern part of the heavens about 15 degrees N. N. E. of the planet Jupiter; and, although they appeared in all parts of the sky, still, if the lines of motion had been continued backwards, they would all have terminated in that point. Having witnessed the meteoric shower of 1833 in Pennsylvania, I was particular to observe the foregoing fact. The phenomenon appeared to me to be identical with that, but far less magnificent. The day preceding had been remarkably rainy, but the night was clear and still.

"Between four and five o'clock, an *auroral arch* was to be seen in the north, and *streamers* at half-past five."

II. Observations at CAMBRIDGE MASS., published in the *Boston Courier*, Nov. 14.

"At eighteen minutes before four o'clock a large meteor darted from the north. It was quite luminous, and in size apparently equal to half the full moon. This was succeeded by many smaller meteors, and twenty three were counted by me during an hour and a half; several were seen by other persons *in the room*,* which escaped my notice. During this time one was observed of great brilliancy, having a luminous train apparently a yard in length. The lightning† continued the whole time, and there was considerable appearance of Aurora Borealis. W."

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* From this expression it is inferred, that the writer had but a small portion of the firmament in view.

† From light clouds in the S. E.

Cambridge Nov. 13.

III. Observations at YALE COLLEGE.

The preceding day had been rainy, and early the same night the sky was overcast; but before midnight, the firmament became cloudless, and the stars shone with uncommon brilliancy. My expectation of a repetition of the meteoric shower at this place was so slight, that I had made little preparation for observing the heavens, although I looked out frequently after midnight. About half-past three o'clock, finding that the meteors began to appear in unusual numbers, I directed my attention towards the eastern part of the heavens, whence they appeared mostly to proceed, and closely watched the stars from the Great Bear on the north, to Canis Major on the south, embracing in my field of view about one third of the firmament.

It was soon discovered that nearly all the meteors shot in directions which, on being traced back, met in one and the same point near the eye of Leo. For a quarter of an hour from half-past three o'clock, I counted twenty two meteors, of which all but three emanated from the above radiant point. Ten left luminous trains; twelve were without trains; and the three that did not conform to the general direction, moved perceptibly slower than the others. The greatest part shot off to the right and left of the radiant, the majority tending south towards the Heart of Hydra. The next fifteen minutes afforded but seven meteors, and the number gradually declined until daylight.

The exact position of the radiant was near a small star forming the apex of a triangle with the two bright stars in the face of Leo, having a Right ascension of 145, and Declination of 25 degrees.* Its place therefore was very nearly the same as in 1834, differing only half a degree in Right ascension; and all the phenomena very much resembled those observed that year, except that they were on a scale somewhat inferior.

IV. Observations at NEW YORK. From the *New York American* of Nov. 15th.

"The annual recurrence of this phenomenon being a subject of much interest, the undersigned kept a careful watch on the night of Saturday and morning of Sunday last, and is gratified in being able to announce the re-appearance of this phenomenon with considerable brilliancy.

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* This position of the "radiant,"as observed here in 1833, was in R. A. 150°, Dec. 20°; in 1834, R. A. 144° 30′, Dec. 30° 15′.

"During the evening, but few meteors were observed, but from eight o'clock until near the dawn, successive flashes were observed in the east, supposed by some to be lightning. At eight o'clock, a very beautiful auroral light was seen of a pinkish color. This continued for a short time only, although a general illuminous appearance in the north remained during the night.

"About two o'clock in the morning, several meteors were seen to dart across the Great Bear, and from this time constant watch was kept up until day light. From two to three o'clock, ninety-eight meteors were counted, some being very small, but the greater number of great size and brilliancy, resembling a rocket both in the explosion and trail left behind.—the trails lasting in some instances for nearly two minutes.

"With two or three exceptions, the course of the meteors was divergent from a point in Leo, Declination 20°, Right ascension 150°, nearly. The place of this point was fully confirmed during the night.

"From three to four o'clock, one hundred and fifty meteors were counted, and three hundred in all were enumerated. After this time we kept no account of the number though many more appeared. From the situation of the observer it is probable that more than half escaped notice. Several were seen in the clear light of the dawn; and Jupiter, Venus, and Mars, all shining with great brilliancy, were alternately outshone by these transient rivals. No doubt now exists in the mind of the writer, as to the distinct and peculiar character of the phenomenon; for, though an attentive observer of such matters, he has never seen anything bearing the slightest resemblance to this display, except on the night of Nov. 12-13th, 1832, when he had the good fortune to observe the same appearance while at sea, off the harbor of Pernambuco, one year before the far famed shower of 1833. G. O. S."

V. Observations at NEWARK, NEW JERSEY. From the *Newark Daily Advertiser.*

This account much resembles the foregoing, as might be expected from the proximity of the two places of observation. The writer remarks, that previous to two o'clock a few shooting stars were seen, but no more than on ordinary occasions. After that however, there was a decided increase. In an hour and a half he counted about seventy five, although his field of view took in only 60 degrees. After four o'clock, their succession was less frequent, and they continued to diminish in number until the dawn of day. He thinks the whole number that fell was not less than four hundred.

VI. Observations at RANDOLPH MACON COLLEGE, VIRGINIA. By *Prof. R. Tolefree,* (communicated in a letter to the writer of this article).

"On the night of the 12-13th November, three of the students and myself prepared to watch all night. The sky was serene and all was calm. About ten o'clock meteors began to appear. The first distinguished for its brilliancy, started from the lower part of the Little Bear and proceeded to the southwest. After midnight until two o'clock, all the meteors shot westward; and from two o'clock until day break their course was entirely northwest. We only watched occasionally during the night, and only on the northern side of the heavens, except an occasional visit to the other parts of the building.* I counted two hundred and forty eight shooting stars, and my companions saw a larger number than this. You may safely conclude that five hundred were seen by us, and this from observations kept up only at intervals during the night."

VII. Observations made in SOUTH CAROLINA. From the *Charleston Courier* of Nov. 25.

"*Greenville, Nov. 19th.*—We learn that the people in the neighbourhood of Maybinton, Newbury District, witnessed the fall of an immense number of meteors, which first made their appearance about twelve o'clock on Saturday night last, and continued their descent until daylight the next morning. It is said their number was not near so great as that of the "Falling Stars" three years since; but the spectacle is represented as having been very brilliant and unusual.

From the foregoing accounts compared, we are led to conclude that the meteoric shower increased in intensity from north to south, that of South Carolina having been the most considerable of all, so far as accounts have reached us.

Does not the recurrence of this phenomenon for six successive years at the *same period of the year,* plainly show its connexion with the progress of the earth in its orbit? and does not the fact that the greatest display occurs every where in places differing widely in longitude, at the *same hour of the day,* as plainly indicate its connexion with the motion of the earth on its axis? The supposition of a body in space, consisting of an immense collection of meteors, stretching across the earth's orbit obliquely, so that the earth passes under it in its annual progress, while places on its surface lying westward of each other are successively brought by the diurnal revolution to the point of nearest approach, will satisfy both these conditions. I can think of no other that will. The "point of nearest approach" may be merely the extremity, or the *skirt* of the nebulous body, while the greatest part of it, and consequently its centre of gravity, lies too distant from the earth to be much influenced by its gravity. It would not be at all inconsistent with the known extent of astronomical bodies, to give to the body in question a breadth of thousands, and a length of millions of miles.

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* Had Professor Toletree taken his station where his view of the firmament would have been unobstructed, he wonld probably have seen a still greater number shooting to the southwest.

It was an accidental observation, made after the conclusion was formed, which ascribes the origin of the meteoric showers to a revolving nebulous body, that first led me to suspect the *Zodiacal Light* to be the body in question. This, according to La Place, *is* such a nebulous body, revolving around the sun in the plane of the solar equator.* We actually observe it to reach over the orbit of the earth, making an angle with its plane of only 7¼ degrees. It is not difficult to place it in such a situation that the earth shall come very near to the *skirts* of it at least. We should, indeed, expect this meeting of the two bodies to take place at the nodes of the solar equator, and therefore in December and June instead of November and April. It is easily conceivable, however, that the aphelion of the Zodiacal Light, at which place it approaches nearest to the earth, does not lie exactly at the node, but so far from it that the earth passes it a month before it comes to its node, at which time, moreover, the earth is more than a million of miles nearer to the sun than its mean distance. In endeavouring to fix the *periodic time* of the meteoric body, since it must be either a year or half a year, (for no other periodic times could bring the two bodies together at intervals of a year,†) several considerations induced the belief, that *half a year* was the true period—an inference drawn especially from the apparent great excess of velocity of the earth at the point of concourse; but the period of *a year,* (or more probably, a little less than a year,) by implying that the two bodies are always comparatively near to each other, would better explain the occurrence of shooting stars at all seasons of the year, and would be particularly favourable to the explanation of those meteoric showers which have, on two occasions, at least,* occurred near the last of April,—a time distant about half a year from November, and therefore sustaining a like relation to the opposite point of its orbit. In such a case, meteoric showers would occur in April and November, for the same reason that the Transits of Mercury take place in May and November exclusively. The greater frequency of meteors in November than in April, naturally results from the greater proximity of the earth to the sun at the former than at the latter period; to which, perhaps, may be added, the effect of the eccentricity of the orbit of the meteoric body, the aphelion being on the side of November. In the present state of our knowledge on this subject, I regard it as a point open for inquiry, whether it will best accord with all the phenomena of shooting stars, to give to the meteoric body a period of nearly one year, or of half a year.

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* Mec. Celeste, (Bowditch) Vol. II. 525.

† See Vol. XXVI. p. 166, of this Journal.

I have been somewhat disappointed, that astronomers should have paid so little attention to the remarkable changes which take place in the Zodiacal Light, about the 13th of November, as has been repeatedly mentioned in this Journal. It appears to me a fact deserving their attention, that the Zodiacal Light, which for weeks before the 13th of November, appears in the morning sky, with a western elongation of from 60 to 90 degrees from the sun, (while up to that time not a glimpse of it can be caught in the evening sky,) should immediately afterwards appear after the evening twilight in the west, and rapidly rise through the constellations Capricornus and Aquarius, to an elongation of more than 90 degrees eastward of the sun, while it as rapidly withdraws itself from the morning sky, and within a few days vanishes entirely from the western side of the sun. For three years past I have observed these changes with much interest, and feel warranted in asserting, that they have been repeated with uniform regularity. The present year, the light was very feeble in the morning sky, an effect partly owing to the presence and peculiar splendor of the planet Venus; but as soon after the 13th of November as the absence of the moon would permit observations, the light appeared in the west immediately after twilight, crossing the Milky Way, and rising in a pyramid almost as bright as that, the triangular space between it and the Galaxy, embracing the Dolphin, appearing, by contrast, strikingly darker.

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* In Virginia, and various other parts of the United States, in 1803, and in France in 1095. Making suitable allowances for the more rapid progress of the earth through the winter signs, and for the change of style, and the Meteoric shower of the 25th of April, 1095, occurred at very nearly the opposite point of the earth's orbit.

I can account for this great and rapid change of place in the Zodiacal Light, a change which is unlike any it sustains at any other period of the year, only by supposing, that on or about the 13th of November it comes very near to us, and that we pass rapidly by it, thus giving it a great parallactic motion, an effect which is in perfect accordance with all our previous conclusions.

According to this view of the subject, *the Zodiacal Light would no longer be regarded as a portion of the sun's atmosphere, but as a nebulous or cometary body, revolving around the sun within the earth's orbit, nearly in the plane of the solar equator, approaching, at times, very near to the earth. and having a periodic time of either one year, or half a year, nearly.*

Such, I affirm, would be the fact, should the Zodiacal Light be proved to be the body which affords the meteoric showers.

Yale College, Dec. 19, 1836.

XXV. *Electro-Magnetic Apparatus and Experiments.* *By* Charles G. Page, M. D.*

Salem, (Mass,) August 23, 1837.

I have completed the following pieces of electro-magnetic apparatus for exhibiting the rotation of conductors by magnets, without the use of mercury. The motory force in such experiments is very feeble, but by the use of solid conductors, as in fig. 28, Plate IV. and I attain a more rapid movement than when the wires run in mercury. The discovery alluded to in the article on the electro-magnetic engine, viz: the admissibility of oil between conducting surfaces, I conceive to be of great importance, and will, doubtless, soon change the whole aspect of electro-magnetic and dynamic apparatus. It supersedes the use of mercury, where freedom of motion and the constant passage of the galvanic current is required.

Fig. 28, represents the ring of De la Rive, mounted for rotation between the poles of a horse-shoe magnet. The ring *a*, is four inches in diameter, and consists of eight turns of copper wire, covered with cotton. Its two ends are brought down at *b b*, and soldered to cylindrical segments of silver.

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* From Silliman's American Journal, for October 1837.

These segments are secured upon, and insulated from the axis. Both are to be reduced in size as much as is consistent with strength, in order to diminish the friction. The two conducting wires, connected with a pair of plates by the mercury cups on the stand, are bent into a spiral at *c* and *d*, in order to press them with a slight spring against the segments *b, b*. When the instrument is used, a drop of oil is put on the segments. This is a pleasing experiment; the ring (if highly coloured) revolving so rapidly, gives the appearance of a hollow sphere. Indeed it would be easy to exhibit two or more concentric rings, revolving different ways, and with different degrees of velocity. I believe this is the first instance of the rotation of a conductor, effected by reversing its tangential action.

Fig. 29 represents the electro-dynamic cylinder of Ampere, mounted in the same manner as in the ring, fig. 28. This helix of wire (called by some the pure voltaic magnet) has its ends bent inward, passing through its axis and brought out at its centre, to be soldered to the segments *b, b*. The arrangement otherwise is similar to fig. 28. The hitherto mode of exhibiting the magnetic polarity of this cylinder, has been to float it, with a battery, in a large basin of acid and water. This inconvenience might have been obviated by mounting the helix as in the figure, and allowing its ends *b, b*, to descend into separate mercury cells; but the use of mercury is objectionable, and by adopting the arrangement described, the polarity of the helix is exhibited in a convenient and pleasing manner.*

Remarks on the preceding paper.

Dr. Page's apparatus are exceedingly ingenious and we have no doubt of their operating well. There is also much novelty in the arrangement; although "the rotation of a conductor, effected by reversing its tangential action" has long ago been performed by other kinds of apparatus. In the *Mechanic's Register* for the year 1825, there is a description of Mr. Marsh's *thermo-electric rectangle*, whose rotatory motions depend upon a *reversal* of the "tangential action." The same instrument, and also a *single rectangle*, operating in the same way, are described in Professor Cumming's "Electro-dynamics," published in 1827, and well illustrated by figs. 96 and 97, Plate VI., of that excellent work. We may also refer our readers to fig. 10, Plate III., of the "Transactions of the Society of Arts, &c." for 1825, for a representation of an instrument which operates upon the same principle. The following is a description of an instrument whose rotations are accomplished by a reversal of the electric current in the moving conductors.

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* Dr. Page has described another method of fitting up the circular coil; but for want of room in the plate for the illustrative figure which accompanies the description, we are obliged to defer presenting it to our readers till the next number. Edit.

In fig. 31, Plate IV., *a, b, c, d, e, f,* is a rectangular brass wire, open at the lower side at *a, f,* and supported on a neat boxwood pillar, by a fine point soldered to its upper side, as seen in the figure. The point which is placed in a conical metallic hole on the top of the pillar, is the pivot on which the rectangle revolves.

Through the upper part of the pillar is a rectangular hole for the admission of two bar magnets (sometimes three magnets are employed), whose similar poles have the same direction. Lower down, a portion of the pillar is formed into a dish, which is divided into two compartments for holding mercury. The upper portion of the pillar stands in the middle of the dish; the whole of the pillar being one and the same piece of wood. The partitions which separate the compartments, are at right angles to the plane of the magnets, and, consequently, the centre of each mercurial compartment, is in the same vertical plane in which is the axis of the magnets.

The extremities *a, f,* of the rectangular wire, are finely pointed, and bent downwards to be slightly immersed in the mercury, whose surface is more prominent than the wooden partitions which separate the compartments in which it is placed. If now the wires C and Z respectively unite the mercury in the cells with the copper and zinc of a voltaic pair, the current through the upper part of the rectangle will invariably flow in the direction indicated by the arrow. By this current the rectangle would be deflected from the poles of the magnet, in such manner that the end *d, e,* would move towards a spectator looking at the figure. The electro-magnetic force would be sufficient to carry the rectangle forward until the point *f,* came into contact with the mercury which the point *a,* had left; the latter now being in contact with that portion of mercury which *f* had left. The current would now be reversed as regards the wire, but would still continue in the same direction as regards the position of the magnets; hence the tendency to deflection would be in the same direction as before; and the rectangle would receive another impulse which would again reverse its position, and change its connexions with the mercurial cells, or poles of the battery. On this principle rapid rotations are produced.

We have employed rectangles, and also circular rings of twelve inches diameter. But when large we find that two, placed at right angles to each other, answer much better than one alone. It would be needless to say how long we have employed apparatus of this kind: it will be sufficient to say that Charles Payne, Esq., late manager of the Adelaide Gallery of Practical Science, has had one of the instruments, represented by fig. 32, in his possession for more than four years. Mr. Payne's instrument has two rectangles placed at right angles to each other. The magnets are about eight inches long, and the electric current is produced by a single pair of bismuth and antimony, heated by a spirit lamp.

The basin of acid and water which Dr. Page complains of, has long been discontinued by the introduction of a neat apparatus invented by Mr. Marsh. This instrument consists of a glass tube, or cylindrical vessel into which is placed the voltaic pair, with the attached spiral conductor standing above. The tube is fixed in the centre of a dish of cork, and the whole floated on a basin of clean water. This invention is upwards of thirteen years old.

Fig. 30 plate IV. is a represention of an instrument which we have long used for the same purpose. It consists of a light copper dish, (made of thin sheet copper hammered to the proper shape) and a disc of copper and another of zinc, each about the size of a shilling, with the spiral conductor. The ends of spiral wire are soldered to the metallic discs, one to each. The copper disc is placed beneath the zinc, they being separated by a piece of paper. When used, the spiral with its metals, are placed in the centre of the dish, with a little dilute nitrous acid; and the whole floated on a basin of clean water, as seen in the figure.

We are exceedingly glad to observe that Dr. Page has derived so much assistance from the use of *oil* in his apparatus. We have long experienced its utility, especially in electro-magnetic engines, and magnetic electrical machines. We are very far from supposing that Dr. Page was aware that oil had long been employed for this purpose in this country prior to the date of his papers. But we must beg permission to state that the fact was well known to some of the most eminent American Philosophers in the summer of 1836. We shall speak more particularly on this subject in our next number; in which we mean to publish some other of Dr. Page's interesting papers. EDIT.

XXVI. *On the stratification of minerals by Voltaic electricity. By* W. NORRIS, ESQ.

Sir,

In the Annals of Electricity for the present month, I find an interesting article on the "influence of electrical action on clay, by Robert Were Fox, Esq.," in consequence of which I am induced to request your insertion of the following:—

Five years ago, a friend of mine, Mr. John Leathart of Alston, Cumberland, explained to me the outline of an electrical theory of mineral veins. Having been abroad, I had no further communication with Mr. Leathart on the subject, until the beginning of 1836, when I had the pleasure of perusing in manuscript an account of his theory which he was preparing for publication. To his theory of the formation and filling up of mineral veins, he had then added an entirely new theory of stratification; but up to that time he had not performed any experiments, nor was he aware of any having been performed, tending to confirm his opinions; those opinions having been formed by close observation, and reasoning on facts which had been presented to him during many years' experience as a practical miner.

As Mr. Leathart's ideas appeared very bold and novel, I advised him, before taking further steps, to attempt to verify by experiment the principles on which his theory was founded. The experiments of Mr. Crosse being published soon after this, tended to confirm some of his views, and Robert Were Fox, Esq., in some points anticipated his theory of mineral veins. There was still, however, a necessity for further experiments to confirm Mr. Leathart's theory of stratification, and after some trouble and delay, arising from our situation in an insulated country town, we succeeded in constructing a simple galvanic apparatus which would yield a gentle continuous action for many weeks or months if required.

The results of a series of experiments with this apparatus appear completely to confirm the ideas of Mr Leathart that the stratification of the earth has been effected by electric agency.

Portions of different rocks were reduced to a fine powder and mingled together with water into a homogeneous mass, of the consistence of soft clay or mud. After being subjected to the action of a current of electricity from ten days to a fortnight, these compounds were separated into distinct layers or strata of the different rocks which had been mixed together; the line of division between the strata being at right angles to the direction of the current.

A variety of interesting phenomena were observed in the course of these experiments. In one case blue limestone was separated from an equal quantity of argillaceous matter, with which it had been mixed, and was converted into white marble. In another experiment portions of carboniferous (blue limestone), siliceous (brown freestone), and argillaceous (plate or shale) rocks were mixed together. The limestone was collected at the positive or zinc end, the plate at the negative, and in the centre the freestone formed a stratum having the appearance of white quartz. But it is not my intention in this notice to enter into particulars. I am in daily expectation of a packet from Mr. Leathart, and hope to be able to present a more detailed account of his views and experiments, for insertion in the Annals of Electricity, for March.

I am, Sir,
Your's, most respectfully,
WILLIAM NORRIS.

4, *Arundel Street, Strand,*
London, January 24th, 1838.

XVII. *On Voltaic batteries.* *By* J. Harper, Esq.

My dear Sir,

The note at the bottom of page 1 of the last number of your Annals, directed my attention to Professor Daniel's voltaic battery, described in the second number, article 17, and which I must have been remiss in not paying it that attention at the time, which it demanded.

In now considering more particularly its construction, a question presented itself, whether it could not be simplified, without lessening its utility. The glass tube appeared objectionable as being troublesome in fitting up, and liable to accident, especially as a series of these batteries are required. The improvement which suggested itself is to get rid of the most expensive parts, that is to say, the copper vessel with its collars at bottom and top, and the glass tube; instead of which, to line a porcelain jar with straight sides (such as those commonly used for keeping preserves in), with a piece of sheet copper which need not be soldered. The copper lining to have a copper wire soldered to the upper part for communication with a cup of mercury. The ox gullet can easily be made water tight at bottom by fitting a cork to it, and binding it round with waxed thread; a small glass syphon is then placed with its shortest leg inside the gullet, of such a length as shall nearly touch the cork at bottom, by which the saturated diluted acid is drawn off, and which may be regulated by inserting a small piece of sponge to discharge it even by drops, if necessary; so that the quantity drawn off may equal the supply of fresh diluted acid admitted at top. Such a battery, I conceive, can be fitted up by any experimenter with very little assistance.

Until the use of mercury can be dispensed with altogether, the cups for holding it I have found to answer best when made of brass in form of the section, fig. 32, Plate IV. The cavity is thus reduced, so as to hold little mercury, and be just sufficient to encompass the wire or wires used. I have long discontinued to amalgamize the wires as it makes them very brittle; they do well without.

I remain, &c.,
J. HARPER,

Oxford, January 17, 1838.

XXVIII. *Electrical Society of London.*

Saturday, January 6.—Mr. W. M. Higgins read an introductory paper on the discovery of galvanism, with an account of the experiments and theory of Volta. After a brief allusion to the discoveries made by Galvani. Mr. H. proceeded to explain the several views in support of and against his theory, by Pfaff, Spallanzani, Munro and Valli. Mr. Higgins then described and followed the progressive experiments and reasonings of Volta, particularly those contained in a paper communicated to the Royal Society, and published in their transactions. In conclusion, he suggested the great assistance science would receive by the re-publication of such papers, as well as those of a more modern date, carefully collated from the stupendous and multifarious transactions of the Royal Society.

Mr. Sturgeon read a paper entitled "an investigation into the cause of the fracture of jars during an electric discharge, and into the mode of protecting them," (this paper is inserted in the Annals of this month.)*

Saturday, January 20.—The assistant secretary read a paper received from Andrew Crosse, Esq., describing the nature and progress of his experiments, by which certain insects of the *Acarus tribe* have been produced. A short account of Mr. Crosse's experiments have already been printed in the

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* See page 86.

Annals, Vol. I. page 242, but in this paper (which has been written at the request of the Society) Mr. C. has not only entered very minutely into the nature and progress of his experiments, but he has also furnished drawings and diagrams of all the apparatus used. The straightforward and unassuming manner in which Mr. Crosse has thus communicated such full particulars of his experiments, is, perhaps, the best answer that gentleman could have made to the uncalled for as well as unprincipled attacks with which he has been assailed, and to which he merely alludes in the following words: "It is most unpleasant to my feelings to glance at myself as an individual, but I have met with so much virulence and abuse, so much calumny and *misrepresentation,* in consequence of the experiments which I am about to detail, and which it seems in this 19th century a crime to have made, that I must state, not for the sake of myself, for I utterly scorn all such misrepresentations, but for the sake of truth and the science which I follow, that I am neither an atheist, nor a materialist, nor a self-imagined Creator, but a humble and lowly reverencer of that great being whose laws my accusers seem wholly to have lost sight of."

The paper will, we understand, be printed by the Society in its transactions, otherwise we should have been induced to have given copious extracts.

XXIX. REVIEWS & NOTICES OF NEW BOOKS.

In the last number of the Annals, we had occasion to notice Mr. Leithead's new publication on Electricity; but for want of room we were obliged to limit our remarks to a few particulars only. Since that time, we have read the work again (or rather the *second part,* which, in fact, is the real soul of it), and our good opinion of the author's abilities, and esteem for his performance, are not in the least abated in consequence. The following specimen will give a good idea of the character of the work.

"*Animal Heat; Nervous Influence; Circulation of the Electric fluid through the Nervous System, &c.*

"The connexion between what is commonly called chemical action and the development of electricity, has already been pointed out in the former part of this essay. The importance of the fact, that no chemical action can take place without a disturbance of the equilibrium of the electric fluid, will, however, be more fully appreciated when considered in a physiological point of view.

"During the process of respiration, the oxygen of the inspired atmospheric air enters into combination with the carbon of the blood, by means of which a two-fold method of supplying the animal heat is obtained, viz. first, by the evolution of caloric from the oxygen gas; and secondly, by the disengagement of electricity during the combination of the oxygen and carbon. Now Crawford's theory, relative to animal heat, is founded entirely apon the single phenomenon of the evolution of caloric from the oxygen gas. But although his hypothesis is sufficient for the explanation of most of the phenomena presented by the production of such heat, there are several which could not be accounted for by this means, *ex. gr.* the increased temperature of a diseased part several degrees above that of the blood taken at the left auricle. For such increase of heat we must search for another cause, and this may readily be obtained, since, at the instant of the formation of the carbonic acid gas in the lungs, electricity as well as caloric is disengaged. But it has been denied that there is any increase of temperature in diseased parts, and that there is merely a sensation of heat experienced by the patient. However this may be, our hypothesis is not affected, for the changes that are produced during respiration are strictly chemical: and no one will now attempt to assert that chemical action, how slight soever, can take place without being accompanied by concurrent changes in the electrical state of the bodies acted upon, and consequently there must be a disengagement of electricity. Electricity then, is set free during the process of respiration, and, indeed, during all the other electro-physiological processes, (if the term may be allowed,) such as digestion, &c. Such being the case, can it be for one moment imagined, that a fluid of so subtle a nature, and continually supplied as it must be during so many processes, has not its peculiar functions to perform in the animal economy? And, impressed as we are, with the conviction of its extreme subtlety, is it not natural to infer that it plays a part in the more mysterious functions of our frames? Those who deny this will place themselves in a dilemma, for, since the equilibrium is invariably disturbed during chemical action, and since the processes of respiration, digestion, &c. &c. as before observed, are strictly chemical, they must be prepared to point out a path by which the constant supply of the fluid may be conducted quietly from the body. What, then, are its duties? First, it aids in the supply of heat. And in what mode does it thus act? Simply thus; by its being transmitted from the nerves, the most perfect conductors in the body, to other parts of the frame, and finally to the surface, through less perfect conductors. The result as regards caloric, when electricity is thus transmitted from good conductors through others of inferior conducting power, is too well known to every electrician to require the waste of space for a list of illustrative experiments, for it will readily occur to him that it is by checking the rapid progress of the fluid by means of imperfect conductors, that we are enabled to inflame combustible substances, to fuse wires, &c.

"Again, in the course of the nerves, there are a number of knots, termed ganglions. These, probably, serve to accumulate a quantity of the electric fluid, so that from its passage from these reservoirs along nervous fibres of extreme minuteness, an increase of temperature will result. Dr. Munro's opinion of the use of these ganglions favours this idea. He supposes them to be 'new sources of nervous energy.'

"Since, then, during every moment of our lives, by means of the various electro-physiological processes continually going on, there is a new supply of electricity, the question naturally occurs,—is there a regular circulation of electric fluid through the whole nervous system, as well as a circulation of the blood through its proper channels? Having shewn, in a former chapter, the extreme sensibility of the nerves to the action of electricity during peculiar electrical states of the atmosphere, how much more powerfully must they be affected when exposed to the more direct influence of that fluid? This leads us to the consideration of a subject of the highest importance, that of the '*nervous principle*,' as some authors have termed that intermediate something between the body and the soul, by means of which the mind acts upon the material part of our frame, giving rise to the phenomena of thought and perception, and imparting motion to those muscles which are under the control of the will. But let us not approach such a subject with a mind disposed to speculate, or imagine; for if there is one species of inquiry throughout the wide-extended field of philosophy, in which, more than in another, the imagination is apt to assert its independence of the control of the judgment, it is, perhaps, in that upon which we now presume to enter.

"Let us, then, for a moment, turn our attention to the results of the researches of the celebrated Magendie, and then proceed to apply them to the foregoing theory, and to that under the head of 'Animal Heat.' He states, then, that the origin of motion and that of sensation are totally distinct, although their connexion, in the healthy state, is so very intimate that the two phenomena seem one and the same. In his memoir, read before the Académie des Sciences, in Paris, on the 22d June, 1823, he remarks—'It must be borne in mind that all the nerves in the body originate from the spinal marrow; that two distinct orders of roots are observable, some taking their attachment from the anterior, the others, on the contrary, emerging from the posterior portion.' Now, is not this precisely such an arrangement of the nerves as would be best calculated to promote such an electrical circulation as above mentioned? Again—'I have,' adds Magendie, 'fully demonstrated that the spinal marrow is, as it were, formed of two distinct cords in juxtaposition, the one of which is endowed with exquisite sensibility, whilst the other, almost completely unconnected with this property, seems to be reserved for *motion*, and that this separation exists through the whole extent of the spinal cord.' Such being the fact, it is highly probable that there is an ascending current of the electric fluid, along one portion of the cord, to the brain, affecting the organs of sensation, and a descending current through the other half, influencing the organs of motion; or, perhaps, that there is a negative and positive portion of the cord, the former constituting the agent of sensation, the latter that of motion. Magendie himself suggests the following fact as an additional argument in favour of the electrical hypothesis, viz. that, 'it is at the *surface* of the organs that its properties, as far as regards motion and sensibility, are the better unfolded.' This fact is worthy of remark, for electricity is said to move only along the *surface* of conducting bodies; and Magendie also found that so far from the respective properties of those parts being more striking in the *interior* of their substance, the *central* part of the marrow is quite insensible, and, by stimulating it, no motion whatever can be produced. Lastly, it is probable that the *modus operandi* is partly thus;—First, with regard to motion. The *will* to move a particular organ, and the act of motion, are simultaneous. The same effort (if effort it may be called) that acts upon the mind, at the same instant, causes the formation of a circuit, by means of which the fluid, in an accumulated state, is instantaneously transmitted to the nerves connected with those muscles which are required to be brought into action.

A popular Treatise on Voltaic Electricity and Electro-magnetism; illustrated by numerous interesting Experiments, with the mode of performing the same. By G. H. Bachhoffner, Esq. Author of "Chemistry as applied to the fine Arts," "The Medico-chemical Compendium," &c. &c, And Lecturer on Chemistry to the Artists' Society. SIMPKIN & MARSHALL. *And* E. PALMER, *Chemical and Phil. Instrument Maker,* 103, *Newgate St.*

To convey an idea of the character of this work, we perhaps cannot do better than give the author's preface.

"The author of the following pages, in presenting them to the public, begs leave distinctly to state, that his chief object has been to arrange, for the guidance of those unacquainted with the subject, a series of interesting experiments: together with such practical information as the limits of the work would permit. He therefore wishes it to be understood, that he does not aim at any originality in the subject matter of the work, but that it is merely intended as a popular treatise—designed chiefly for the use of amateurs." A lithographic plate of apparatus attends the work.

XXX. MISCELLANEOUS ARTICLES.

On the Aurora Borealis of July 1*st.* 1837.*

I. Observations made at ROCHESTER, by Professor C. Dewey.

On the evening of July 1st. the aurora borealis was very splendid: indeed it far exceeded the spendour of that of the 25th of January last, as it appeared in this part of the State. The day had been pleasant and warm. About two P. M. the temperature was 86°, and a shower was collecting rapidly in the northwest, which in the next hour and a half had been blown over us and dissipated with little rain. The temperature changed, and the sun shone forth in all his glory. The remainder of the afternoon was delightful. The evening was cool, the temperature being about 58°. Soon as the twilight had ceased, the aurora was seen in short *flocculent* cloud-like forms all across the northern sky. Soon it extended quite round to the east and west points, at both of which broad and bright arches arose and extended more thanhalf way to the zenith, while a multitude of streamers rose all round the

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† From Silliman's American Philosophical Journal, for October 1837.

northern sky towards the same point. About half-past nine the broad belt of brilliant white aurora, rising from both sides of the east point, shot towards the zenith, near which it was met by a corresponding but less brilliant zone of light from the west. The general appearance continued very brilliant till ten minutes after ten, when the point a little south and east of the zenith, and towards which all the streamers and pillars were directed, became a bright rose-red, and soon sent off brilliant corruscations in every direction but the south, with distinct flashes of white light much resembling that which is commonly called *sheet-lightning*. This soon ceased, and the white aurora again appeared as before. Near half-after ten a dark brown aurora arose in the northwest, and extended upwards; soon after appeared on all sides the rose-red or deep crimson, rising to the vertex near to the star Zeta, in the constellation Hercules, nearly in a right line between Alpheus in the Northern Crown and Lyra. The whole expanse except the south was most splendid. Soon the flashing from all sides towards the vertex mentioned, was renewed with great power. Great and constant changes in the colour were occurring. The white beams and streamers intermingled with the red, added to the splendour of the scene; at length the brilliant flashing and waving of the aurora ceased. The vertex became clear of it, except as it flashed up in long and broad waves, and showed in serpentine forms for an instant and then disappeared. Soon, however, the whole scene was repeated. The vertex retained its place, as the constellation moved westward, and was now near Mu, in Hercules, and all the splendid light, beams, pillars, arrows, waving and flashing, were, if possible, more splendid than before. This was at eleven o'clock. The colours were constantly changing their hues, from all the northern, eastern, and western parts, the flashing light rose to the vertex, and seemed to shoot back again as it came. Often the light would flash through thirty or forty degrees, disappear within twenty degrees of the vertex, and reappear flashing as before, for the last ten degrees, as if it passed for ten degrees behind some opake substance. The sky was cloudless for the whole time. At a quarter after eleven the red light disappeared, while long, arrow form, splendid streamers, continued to play for some time till they gradually subsided and only a luminous sky remained for most of the night. On the next evening, there was a slight aurora. Whatever of beauty, splendour, or grandeur, others may have seen in this phenomenon, no aurora has come under my observation of equal brilliance and variety.

Rochester, July, 1837.

II. Observations made at NEW HAVEN and ELSEWHERE.

This very brilliant display of northern lights was witnessed as far south as Columbus, Ga. (lat. about 32° 35′ N., long. 85° 11′ W.) It was seen there for about half an hour, commencing at $9^{h.}$ $30^{m.}$ Many streamers of a red colour were observed, but their altitude is not stated. We have also observations of the phenomenon from Cleveland, Ohio; Fayetteville, N. C. and various places in Virginia, which, so far as they go, substantially agree with those made here. At Richmond the display between two and three A. M. of the next morning, was distinctly noticed by a friend who happened to be there: but the printed statements make no mention of it. The observations below given, were made by several persons of this place, and are in the main the same as were published in the New Haven Daily Herald of July 6, 1837.

E. C. H.

An auroral display of unusual variety and splendour was witnessed in this city on the night of Saturday last, the 1st of July. The day was one of the warmest of the season; at 2 P. M., therm. 84° Fah.; wind S. W. Towards the latter part of the afternoon, dark clouds arose in the northwest and gave a promise of a thunder storm, but about an hour before sunset they passed off to the northeast without much rain. At 6 P. M. therm. 78°, barom. 29·67 inches, wind light and from N. N. W.

At $9^{h.}$ $25^{m.}$ just before the departure of twilight, the northern sky was observed to be faintly illuminated from W. N. W. to N. N. E. but much obscured by clouds. It soon became clear. At $9^{h.}$ $38^{m.}$, streamers began to form in the north, and soon after in N. E. and N. W., gradually becoming more frequent and increasing in brilliancy. At $10^{h.}$ $30^{m.}$ the action was more energetic, and the scene eminently animated and beautiful. From E., N., and W., and all points between, streamers shot up from near the horizon in quick succession, with wonderful celerity, and passed beyond the zenith, while others starting from an altitude of about 30° in the south, met the former in the corona in the constellation Ophiuchus. Auroral waves soon appeared, flashing upwards with great rapidity across the streamers and rolling up in wisps and sheets around the coronal point. The colour of the streamers and waves was mostly a phosphoric white, but about $10^{h.}$ $40^{m.}$ for a short time a fine rose red predominated.

At $11^{h.}$ $10^{m.}$ the display was on the decline. By midnight it became quite faint, and the heavens were at the time much obscured by clouds. About this period the light was mostly confined to the eastern horizon, where among the clouds were seen indistinct columns of red and white. About $1^{h.}$ A. M. July 2nd, the clouds dispersed, and the sky became exceedingly clear, and thus continued the remainder of the night.

At $2^{h.}$ the aurora began to revive, and soon presented a spectacle in many respects surpassing the former. At $2^{h.}$ $10^{m.}$ an indistinct arch about a degree wide, appeared, with vertex about 8° high in the N., between which and the horizon, the sky, although clear, seemed to be covered with dark vapour. From this arch arose broad streamers of a vivid yellowish white. Some of the streamers, however, occasionally started from points in the dark space below the arch. About $2^{h.}$ $30^{m.}$ the display was at its maximum. From W. N. W. to E. the sky was filled with streamers, passing over head, and forming a corona in the constellation Cygnus. Along these columns or streamers, swept upwards immense auroral waves, nearly unbroken from the horizon to the magnetic equator. These columns remained in unabated splendour for fifteen minutes, and were visible until about 3. At $2^{h.}$ $38^{m.}$ the arch was extinct, and the streamers were becoming shorter and less frequent. They were however, for a long time, numerous about the north, and were visible until overpowered by the superior light of the advancing sun. They were distinctly observed as late as $3^{h.}$ $30^{m.}$ or about an hour after day break.

Many observations on the position of the corona were made during the night; those which are the most trustworthy are the following, viz.—

$2^{h.}$ $31^{m.}$	centre of corona,	alt. 75° 25′	azim. S. 4° 27′ E.
39	- - - -	74 55	- - 3 10
42	- - - -	74 40	- - 5 07

These positions correspond nearly to the direction of the dipping needle at this place, if we make due allowance for the perturbations which the aurora may have occasioned, and for the determining with precision the central point.

The horizontal needle was much disturbed. Between $10^{h.}$ $44^{m.}$ and $11^{h.}$ it traversed 3° 4′. In general, the north end of the needle was carried to east of its mean position at this place, which is now about N. 5° 55′ W. After midnight, the range of variation did not exceed one degree. The needle was not observed on the 2nd and 3d inst.

From sun-set to $2^{h.}$ 30^{m} the wind was from N. W. and faint; after that time, from N. N. W. and somewhat stronger. At $11^{h.}$ $40^{m.}$ the dew point was 67°, therm. being at 72°. The barometer rose during the night: at $2^{h.}$ $30^{m.}$ A. M. (2nd inst.)

it stood at 29.76, at 6^{h} at 29.80. Thermometer at 2^{h} 30^{m} 71° at 6^{h} 69°.

It is worthy of notice, that on this occasion there were two well marked and distinct *seasons of greatest brilliancy or fits of maximum intensity*, at an interval of about four hours. It will be found on examination of former accounts, that this is a common feature of auroral exhibitions of unusual brilliancy, and that the first fit occurs within about an hour after the end of twilight. Future observations continued during the entire night, must determine the number of these seasons and the interval between them.

The aurora appeared on the night of Sunday the 2nd inst., and was observed until 1^{h} 30^{m} of the 3d inst. It was not very conspicuous. At 9^{h} 30^{m} there appeared a low dim arch, with vertex about 50° high, sending forth occasional streamers to an altitude not exceeding 30°, after which no special change was noticed. The day was clear and fine: therm. at 2 P. M. 78°.

On Monday night, 3d inst., the aurora was again seen. It was less conspicuous than on the 2nd. The evening was showery, but at 9^{h} 45^{m} the clouds began to disperse. The north was illuminated with a faint light, now and then adorned with a solitary streamer. Observations were continued until near midnight, but no increase was seen.

The hours and minutes above given are of apparent time.

A. C. C. E. J.

Notice of an Aurora Borealis, seen at Burlington, U. S. by James Dean.

On the 29th July, 1837, a luminous arch appeared, commencing 8° or 10° from the eastern horizon: it passed between Alpha and Zeta Pegasi, between Alpha and Beta Lyræ, just north of Arcturus, and terminated 19° or 20° from the west horizon. It was about 3° broad and well defined. Thus I first saw it about 10^{h} P. M. It moved slowly to the south, fading at the extremities: at 10^{h} 15^{m} it passed over Beta Cygna, faint but still well defined for some distance on each side of the meridian, about 2° broad, but soon vanished, the last traces appearing in the head of Hercules. There was at the time a bright light along the northern horizon, but it presented no uncommon features.

It is rather remarkable, that about a month afterwards, a similar arch should occur at exactly the same time in the evening, and occupying very nearly the same place; but it was so. On the 25th of August it was nearly repeated, the eastern part, however, being a little farther north, touching at 10 P. M.

Gamma Pagasi, and in twelve or fifteen minutes moving over Alpha Pagasi, where it vanished: in the west below Arcturus it sloped off to the northwest, making an angle of 45° or 50° with the horizon. The western part disappeared about $10^h\ 15^m$, by spreading. The northern light was brighter and more active than before, but too irregular and unsteady to admit a hope of recognition by others.

Within ten years I have seen four similar phenomena, and much to my disappointment they all evince a stubborn aversion to respecting the magnetic meridian. Burlington is in lat. 44° 28′ N. long. 73° 15′ W.

Silliman's Journal.

Davenport's *Electro-Magnetic Machine.*

Since the notice of this invention in the April number of this Journal,* the proprietors have been engaged in experiments on magnets of different modifications, as well as on the proper distance between the magnetic poles of the circle. The form and arrangement of the magnets have been entirely altered and the energy of the machine greatly increased. The proprietors have discontinued the use of magnets in the form of segments of a circle and now use them in something like the horse-shoe form, changing the poles once in every 3½ inches of the circle. On this arrangement, a machine with a wheel seven inches in diameter, (being but a trifle larger than the one formerly described in this journal) elevates ninety pounds one foot per minute, and will perform about twelve hundred revolutions in the same time.

A machine has also been constructed with a motive wheel one foot diameter, which moves with great energy, but its power has not been tested by the elevation of weights. One of the machines, with a motive wheel only seven inches in diameter, has been attached to a turning lathe, and moves it with astonishing strength, compared with the small size of the propelling engine.

The experiments and improvements hitherto made serve to strengthen the hopes at first entertained in regard to the value and importance of this invention.

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* This number of Silliman's American Journal has not yet come to hand; and on that account we have not given our readers a description of this celebrated machine. We are well aware that much has been said about it in this country, and many erroneous notions have been formed respecting its structure and power. We shall give a full description of it in our next number. Edit.

The proprietors are now engaged in constructing a machine with a motive wheel of about 2½ feet in diameter, from which they expect to obtain sufficient power to propel a Napier printing press.

For the purpose of raising funds to carry on experiments, &c., a joint stock association has been formed in New York, of which Mr. Edwin Williams, No. 76, Cedar Street, is agent. By this arrangement the principal interests of the patent for the United States and Europe, being placed in a stock of 3,000 shares, the proprietors offer an opportunity to public spirited individuals to become associated with them in the enterprise, which it is hoped, for the benefit of mankind, may prove successful. A sufficient number of shares, we learn, have been already taken to provide ample funds for experiments on a large scale, and the public with interest wait the result.

Silliman's Journal.

A Pamphlet on Electro-Magnetism.

A pamphlet of ninety-four pages has been published in New York, containing a history of Davenport's invention; notices of it from periodical publications; and a summary of our knowledge upon the subjects of electricity, galvanism, electro-magnetism, &c., by Mrs. Somerville. If the anticipations of some of the journalists appear extravagant; the summary of Mrs. Somerville, replete as it is with the most interesting and astonishing facts, may well account for the strength of impression produced on the minds of observers by the inexplicable movement of a machine, whirling round with vast rapidity, while there is no obvious cause; and the real cause, when pointed out, appears so inadequate to the effect.

We rather regret that this interesting application of electro-magnetism is attempted to be sustained by the appeal of the hope of immediate profit. Surely there are not wanting men, and we trust they are numerous, who will cheerfully pay, and, if necessary, cheerfully lose, the comparatively small sums, whose considerable aggregate will carry forward this interesting research, until the ratio and the extent of its power are ascertained; and, if it should prove that the limit is far beyond the demands of practical application, so much the better: but neither the ratio nor the extent can be learned without persevering experiments, the expense of making which, and of sustaining all who are concerned in making them, will be, we trust, cheerfully borne by the public.

Silliman's Journal.

Cold produced by liquid carbonic acid in its transition from the liquid to the gaseous state.

When a jet of the liquid acid is directed upon the bulb of an alcoholic thermometer, it rapidly sinks to —90^{c} Cent. But the frigorific effects do not respond to this abasement of temperature, a fact which is explained by the almost absolute want of conducting power of the gases and their low capacity for heat; hence the *intensity* or *tension* of cold is enormous, but the sphere of activity is limited in some sort to the point of contact. The congelation of the mercury is confined to small portions of it, and if a finger is exposed to a jet of the liquid a sensation of burning is indeed forcibly felt, but the effect is chiefly confined to the epidermis.

If gases have little effect in the production of cold, it is not so with vapours, whose conductibility and capacity for heat are much greater. I have therefore thought that if a permanent liquid—ether, for example—could be placed under the same condition of expansibility as liquefied gases, we might obtain a frigorific effect much greater than that procured by liquefied carbonic acid. To accomplish this, ether must be rendered *explosible,* and this I have easily effected by mixing it with liquid carbonic acid. In this intimate combination of the two liquids, which dissolve each other in all proportions, ether ceases to be a permanent liquid under atmospheric pressure; it becomes expansible like a liquefied gas, still preserving its properties as a vapour—viz. its *conductibility* and capacity for caloric.

The effects produced by a blowpipe fed by explosible ether are remarkable: a few seconds are sufficient to congeal fifty grammes of mercury in a glass capsule. If we expose a finger to the jet which escapes from this *veritable blowpipe of frost,* the sensation is quite intolerable, and seems to extend much farther from the point of contact than with the liquid jet.

I propose to replace ether by *carburet of sulphur,* which will in all probability produce still more striking effects.—(Annales des Chim. Decem.)

Silliman's Journal.

Fig. 33

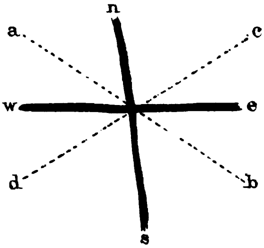
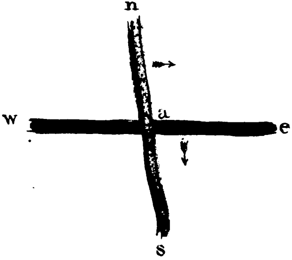
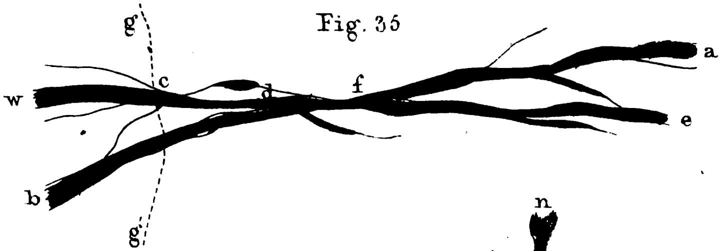
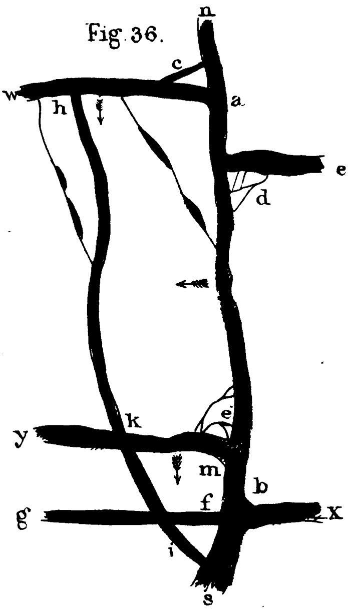
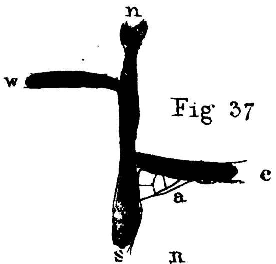


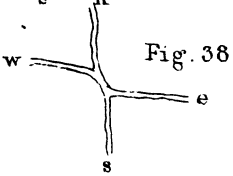
Fig. 34.











Copper Veins ▬

Tin D^o ▬

Cross Courses in horizontal Plans …. .. ‖ n s

Horizontal Plans.

W. Annan lithog. 12. Gracechurch S^t.

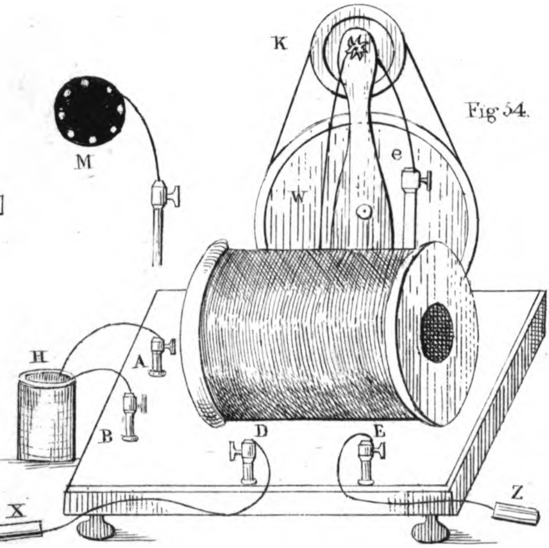


Fig. 54.

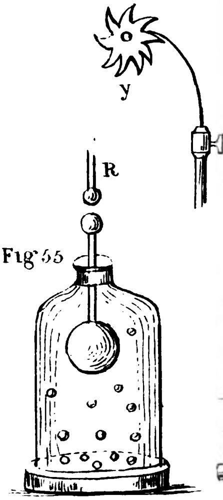


Fig. 55

Fig. 49.

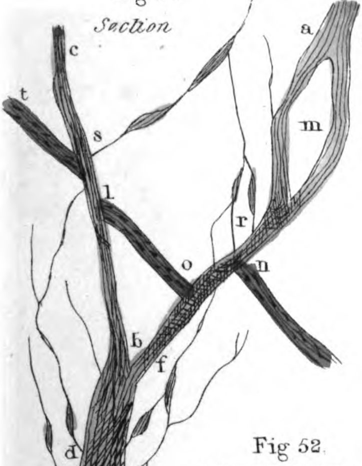
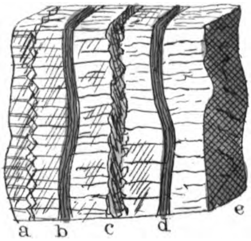


Fig. 50.



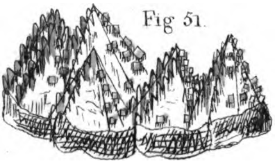


Fig. 51.

Fig. 52.

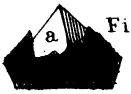


Fig. 53.

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ANNALS OF ELECTRICITY &c.

PLATE VI. Vol 2.

Fig. 39

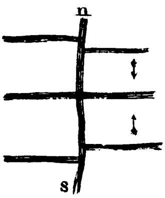


Fig. 41.

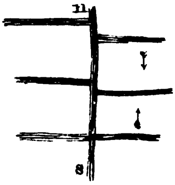


Fig. 43.

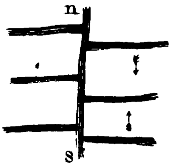


Fig. 40

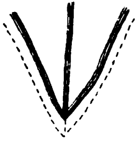


Fig. 42

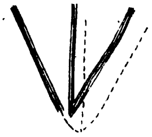


Fig. 44.

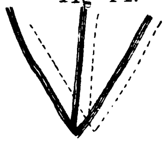


Fig. 45

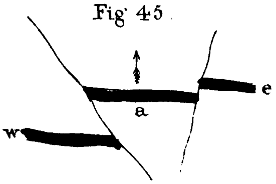


Fig. 46.

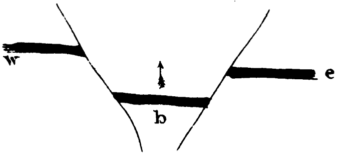


Fig. 47

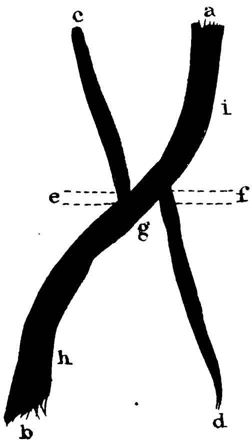
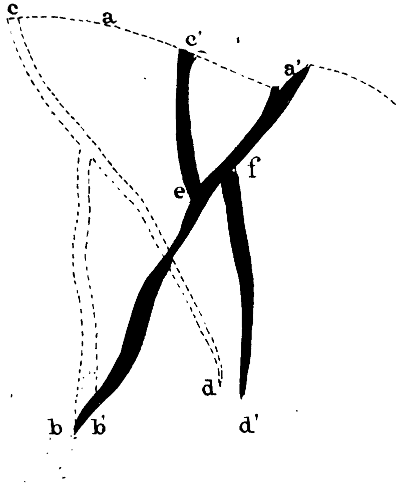


Fig. 48.



Figs. 39. 41. 43. 45 & 46. Ground Plans.
Figs 40. 42. 44. 47 & 48. Sections.

W. Annan lithog. 12. Gracechurch St.

THE ANNALS
OF
ELECTRICITY, MAGNETISM,
AND CHEMISTRY;
AND
Guardian of Experimental Science.

MARCH, 1838.

XXXI. *Of the Reaction of the Essential Oils with Sulphurous Acid, as evolved in union with Ether in the process of Etherification, or otherwise. By* R. Hare, M.D. *Professor of Chemistry in the University of Pennsylvania.*

Having mixed and subjected to distillation two ounces of oil of turpentine, four ounces of alcohol and eight ounces of sulphuric acid, a yellow liquid came over, having all the appearance of that which is obtained in the process for making oil of wine, described in the preceding article.* On removing, by means of ammonia, the sulphurous acid existing in the liquid, and driving off the ether by heat, a liquid remained, which differed from oil of turpentine in taste and smell, although a resemblance might still be traced. This liquid was without any sensible action on potassium, which continued bright in it for many weeks. It proved, on examination, to contain a small quantity of sulphuric acid. I ascertained, afterwards, that in order to produce these results, it was sufficient to pour oil of turpentine on the mass which remains after the termination of the ordinary operation for obtaining ether, and apply heat. Subsequently it was observed that when the sulphurous ether was removed by heat or evaporation, without the use of the ammonia, the proportion of sulphuric acid in the remaining oil was much greater.

By subjecting to the same process several essential oils, I succeeded in obtaining as many liquids to which the above remarks were equally applicable. With some of the oils, however, similar results were, by this method, either totally or partially unattainable, in consequence of their reaction with the sulphuric acid being so energetic as to cause their decomposition before any distillation could take place. No product can be obtained by distillation with sulphuric acid and alcohol from the oil of cinnamon obtained from cassia. From the oils of sassafras and cloves, but little can be procured.

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* See Article XIV., p. 103, last number.

However, in one instance, by previously mixing the oil of sassafras with the alcohol, in the manner described in the account given of the first experiment with the oil of turpentine, I succeeded in obtaining, in addition to a small quantity of the heavy liquid containing sulphuric acid, a minute quantity of a lighter one, devoid of that acid, which burned without smoke, was insoluble in water, and very fluid. I am disposed to consider the liquid thus procured as a hydrate of sassafras oil, or sassafreine, as I would call it, being analogous to hydric ether.

The oil of sassafras, whether isolated or in combination, possesses a remarkable property, which, I believe, has not attracted sufficient observation : I mean that of producing an intense crimson colour, when added, even in a very minute quantity, to concentrated sulphuric acid.

One drop of oil of sassafras imparted a striking colour to forty-eight ounce measures of sulphuric acid, and appeared perceptible when it formed less than a five millionth part. This property was completely retained by the lighter liquid above described as procured from oil of sassafras.

I subsequently observed, that when sulphurous acid, whether in the form of sulphurous ether, in that of a gas, or when in union with water, was brought into contact with any of the essential oils (including kreosote), which were subjected to the experiment, they acquired a yellow colour, and a strong smell of this acid.

In the case of the yellow compound thus obtained from any of the essential oils which I have tried, if the sulphurous acid be removed by heat, the oil, by analysis, will be found to yield sulphuric acid. That some acid of sulphur remains in union must be evident, since washing with ammonia will not entirely remove the power of yielding sulphuric acid; and the total absence of the sulphurous smell demonstrates that the sulphurous acid either enters into an intimate combination with the oil, or acquires oxygen sufficient to convert it into sulphuric or hyposulphuric acid.

Those essential oils which contain oxygen, are most affected by the action of sulphuric acid.

Both the oils of cloves and cinnamon, after admixture with sulphurous ether and subsequent distillation, gave, on analysis, precipitates of sulphate of barytes. In the case of cloves, the precipitate amounted to one-seventh of the whole weight.

By distilling camphor with alcohol and sulphuric acid, I obtained a yellow liquid, which, by washing with ammonia and evaporation, in order to get rid of the sulphurous ether, yielded an oil. The oil, by standing, separated into two portions, one solid, the other liquid. The solid portion resembled camphor somewhat, in smell, but differed from it by melting at a much lower temperature, becoming completely fluid at 175°

I found that the essential oils of cinnamon and cloves possessed an antiseptic power, quite equal to that of kreosote, and that their aqueous solutions, when sulphated, were even superior to similar solutions of that agent.

One part of milk mingled with four parts of a saturated aqueous solution of the sulphated oil of cloves, remained after five days sweet and liquid, while another portion of the same milk became curdled and sour within twenty-four hours. Having on the 2d day of July added two drops of oil of cinnamon to an ounce measure of fresh milk, it remained liquid on the 11th; and, though it finally coagulated, it continued free from bad taste or smell till September, although other portions of the same milk had become putrid. A half ounce of milk, to which a drop of sulphurous oil of turpentine had been added, remained free from coagulation at the end of two days, while another portion, containing five drops of pure oil of turpentine, became curdled and sour on the next day.

A number of pieces of meat were exposed in small wine glasses, with water impregnated with solutions of the various essential oils. Their antiseptic power seemed to be in the ratio of their acridity. The milder oils seemed to have comparatively little antiseptic power, unless associated with the sulphurous acid, which has long been known as an antiseptic.

In cutaneous diseases, and, perhaps, in the case of some ulcers, the employment of the sulphurous sulphated oils may be advantageous.

A respectable physician was of opinion that the sulphurous sulphate of turpentine had a beneficial influence in the case of an obstinate tetter.

Possibly the presence of sulphurous acid may increase the power of oil of turpentine, as an anthelmintic.

Pieces of corned meat hung up, after being bathed with an alcoholic solution of the sulphurous sulphated oil of turpentine, or with solutions of the sulphated oils of cloves or cinnamon, remained free from putridity at the end of several months. That imbued with cinnamon had a slight odour and taste of the oil.

I am led, therefore, to the impression that the antiseptic power is not peculiar to kreosote, but belongs to other acrid oils and principles, and especially to the oils of cinnamon and cloves.

The union of sulphuric acid with these oils appears to render them more soluble in water; whether any important change is effected in their medical qualities by the presence of the acid may be a question worthy of attention.

I have stated my reasons for considering the ammoniacal liquid, resulting from the ablution of the ethereal sulphurous sulphate of etherine with ammonia, as partially composed of hyposulphuric acid. By adding to this ammoniacal liquid a quantity of sulphuric acid, sufficient to produce a strong odour of sulphurous acid, and then a portion of any of the essential oils; a combination ensued, as already described, between the oils and the sulphurous acid liberated by the sulphuric acid, so as to render them yellow and suffocating. The habitudes of cinnamon oil from cassia under these circumstances were peculiar. A quantity of it was dissolved, communicating to the liquid a reddish hue. The solution being evaporated, a gummy translucent reddish mass was obtained, which, by solution in alcohol, precipitated a quantity of salt, and being boiled nearly to dryness, re-dissolved in water, and again evaporated, was resolved into a mass having the friability, consistency and translucency of common rosin; but with a higher and more lively reddish colour. Its odour recalls, but faintly, that of cinnamon; its taste is bitter and disagreeable, yet recalling that of the oil from which it is derived. Its aqueous solution does not redden litmus; nor, when acidulated with nitric acid, does it yield a precipitate with nitrate of barytes.

Of this substance ten grains were exposed to the process above mentioned, for the detection of sulphuric acid, and were found to yield a precipitate of 6·5 grains of sulphate of barytes.

It may be worth while to mention, that in boiling the sulphated oils with nitric acid, compounds are formed finally, which resist the further action of the acid, and are only to be decomposed by the assistance of a nitrate and deflagration. I conjecture that these compounds will be found to merit classification as ethers formed by an oxacid of nitrogen.

One of my pupils, in examining one of the compounds thus generated, was, as he conceived, seriously affected by it, suffering next day as from an over dose of opium. He also conceived that a cat, to which a small quantity was given, was affected in like manner.

I had prepared an apparatus with the view of analyzing accurately the various compounds above described or alluded to, by burning them in oxygen gas; when, by an enduring illness of my assistant, and subsequently my own indisposition, I was prevented from executing my intentions.

XXXII. *Of Sassarubrin, a Resin evolved by Sulphuric Acid from Oil of Sassafras, which is remarkable for its efficacy in Reddening that Acid in its concentrated state. By* R. HARE, M. D. *Professor of Chemistry, in the University of Pennsylvania.*

This colour is due to a peculiar resin, which I would call sassarubrin, being elaborated from the oil of sassafras, by its reaction with sulphuric acid, with phenomena which are striking, and, in some respects, singular. If a mixture be made of equal parts of the oil of sassafras, alcohol and sulphuric acid, on raising the temperature to a certain point, the whole mass rises up in a resinous foam, of a beautiful colour, between copper and purple, with a metallic brilliancy. In some instances, it has been partially forced out of the retort through the beak in a cylindrical mass, which acquired, on cooling, the consistency of pitch. This pitchy substance is a compound of the resin above alluded to and sulphuric acid, with which it forms a soluble substance, neutralising its sourness to a certain extent. By steeping this subacid compound in ammonia, straining, washing the residue with water, and desiccation, a brittle tasteless resin remains, which is quite insoluble in water, but very soluble in alcohol and hydric ether.

The addition of this sassarubrin to concentrated sulphuric acid, produces the crimson colour already mentioned as resulting from the presence in that liquid of a minute portion of oil of sassafras. I infer that the colour is due to the evolution of sassarubrin, which has a bassic affinity for the acid, to which it owes its birth. The ethereal and alcoholic solutions of sassarubrin are of the colour of a dingy white wine, but acquire a deep crimson when mingled with concentrated sulphuric acid.

Sassarubrin may be produced by the union of the acid and oil, provided it be moderated by refrigeration or dilution with water.

Without some precaution, the heat produced is sufficient to char the resin more or less. The reddening influence of the oils of cinnamon and cloves is due to the generation of resins analogous to sassarubrin.

To those resins the names of cinnarubrin and clovorubrin may be severally assigned. Cinnarubrin may be evolved by adding oil of cinnamon to equal parts of sulphuric acid and water, previously mixed and refrigerated, the temperature being subsequently elevated till the mass rises up in a foam; when the whole should be poured into a solution of pearlash, from which the resin may be extricated by a strainer. It is analogous to sassarubrin, but is less efficacious in colouring sulphuric acid, and does not, like the former, impart to the sides of the containing glass a rich red colour. Moreover, it appears to be partially insoluble in alcohol, and to retain sulphuric acid after being boiled with an alkaline solution.

I infer that a new series of resins may be evolved from the essential oils by their reaction with sulphuric acid; which, having a general analogy to each other, may still have discriminating characteristics, arising from the oils whence they may be derived.

XXXIII. *Theoretical Views of the origin of Mineral Veins. By* R. Were Fox, Esq.

I propose now to consider how far the phenomena of mineral veins can be accounted for on known principles. I am aware that it may require much time and research fully to solve the problem, but I trust that I shall succeed in making out a primâ facie case in favour of the probability of its being ultimately accomplished.

I have long been impressed with the analogy which mineral veins seem to present to some voltaic combinations, and have referred to it on various occasions. In one of my papers, "On the temperature of mines," which was read before the Cornwall Geological Society, in 1822, I adverted to the subject in these terms:*

"If electricity, for instance, be evolved when several different mineral substances are brought into contact, and likewise in the process of crystallization, &c., may it not in connexion with the strata and veins, and the almost distinct portions of water which abound in the earth, also act its part on a larger scale, and not only excite heat.† but contribute to produce the extraordinary aggregation and position of homogeneous minerals in veins, &c., and the beautiful order which exists even under the surface of the earth?"

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* The paper from which the extract is made was published in the Annals of Philosophy in 1822. See Vol. IV. p. 447.

† I am quite inclined to believe that there is an independent source of heat in the interior of the earth, although the circulation of water under the surface, and its tendency when heated to ascend,

In 1827, I again alluded to the subject, and to the apparent analogy between electro-magnetism and the generally prevalent direction of the principal metallic veins, nearly at right angles to the magnetic meridian. Between two and three years afterwards I commenced my experiments on the electro-magnetic properties of metalliferous veins, and proved the reality of the existence of electricity in them.

Scarcely a year, however, has elapsed since my opinions, with regard to the formation of mineral veins, have assumed a shape, definite enough in any degree to warrant my communicating them to the Geological Society of London, which I did last Spring; and I have since availed myself of other occasions to enter more fully into some parts of the question.

Formation of Fissures.

I am aware that the prevailing opinion in Cornwall, is rather opposed to the hypothesis of mineral veins having been derived from fissures in the strata; nor can I be surprised at it, as I have participated in the same opinion. I could not conceive how numerous, large, and deep fissures could have remained open, during the formation of mineral deposits in them, under the circumstances in which the veins are now found to exist, since they intersect each other in various directions, and their vein stones, when included in a given rock, do not often resemble, or appear to have belonged to any other rocks immediately above, when traversed by the same veins. For reasons such as these, I refrained from adopting any general theory, as those of Werner and Hutton seemed to me to be very unsatisfactory, and indeed inconsistent with many facts.

My objections to the hypothesis of fissures, were, however, removed, when it occurred to me that many of them might have been very small at first, and become progressively opened and filled with mineral deposits. Moreover, that other secondary and lateral fissures might have resulted from time to time, from the expansion of the former; which, I think, obviates any mechanical objections derived from intersections, and from the fact that contiguous veins often include large masses of rock detached and isolated from the neighbouring strata, or "*country.*"

must, I think, render any inferences founded on experiments in mines inconclusive as it respects the true ratio of the increase of temperature at very great depths.

The circumstance of vein stones derived from any given rock, not being found in a vein, whilst traversing an inferior rock, appeared to be easily accounted for, if the fissure, instead of being wide, were contracted enough materially to check the descent of small fragments of rock: and the very frequent subdivision of the larger veins into smaller ones, seemed, moreover, to afford decisive evidence in favour of such a process.*

I admit that the mineral veins in Cornwall, are not more liable to hypothetical objections, such as I have alluded to, than those which occur in fossiliferous rocks, and I should long ago have been satisfied on this point, had I been sufficiently acquainted with the strict analogy, which has been shown to exist between them, in what may be considered their mechanical characters. But it is not now necessary to refer to such evidence, at a distance, since De La Beche has discovered encrenites, and other organic remains, imbedded in killas, (grauwackè,) close to the walls of the eastern part of Great Crinnis copper and tin lode; and this gentleman, moreover, seems to entertain no doubt from the direction of some of the fossiliferous beds, that they must pass under many of the copper and tin mines near St. Austell.

It may not be easy to determine what has given rise to fissures in the earth, but it is likely that different causes have operated, some probably sudden and violent, and perhaps often repeated, producing, at various intervals of time, considerable disruptions of the strata; whilst others may have been more slow, and gradual; but even in this case, I conceive, that the effects upon any given fissure would mostly be intermittent, or by fits and starts; for it may readily be imagined that if the influence of tension were exerted to a certain degree, there would generally be a sudden enlargement of the rent.

Earthquakes, which are even now of such very common occurrence in some countries, may give some idea of the former; and the gradual elevation or depression of vast tracts of land, which is found to have taken place in different parts of the world, even in modern times, may be mentioned as an exemplification of the latter. These changes of level may, perhaps, be owing to fluctuations of temperature under the earth's surface.

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* Since I first published my views on this subject, I have learnt that Fournet, a French geologist, has observed in the mines of France, &c., proofs of the progressive enlargement and filling of fissures, so that the circumstance of our having both arrived at the same conclusions by independent observations, in widely separated mining districts, is certainly favourable to the truth of the hypothesis, as well as to its generality.

The phenomena of mineral veins in Cornwall, prove that the various rocks which are traversed by them, must have occupied their present relative positions before the fissures were produced; and I conceive, moreover, that none of these rocks could have been at the time, at temperatures greatly differing from each other, because the veins which traverse them at various angles and inclinations, are neither dislocated nor necessarily altered in size, in passing from one rock into another, being sometimes larger in one kind of rock, and sometimes in another. These remarks apply to the veins, even when they pass through elvan courses which traverse other rocks; whereas, if any of the rocks were at a much higher temperature than those in their vicinity, after the formation of the fissures, the contraction caused by the cooling of the former, surely ought, under some circumstances, to have interrupted the continuity of veins, and to have increased their dimensions.

I think we may, therefore, venture to assume, that fissures have not been produced by any cooling of the rocks from a high original temperature, but that they must be attributed to the operation of other causes.

It is exceedingly probable, however, that changes or alternations of temperature under the surface, may have been one of the processes from which some fissures have resulted, and were afterwards expanded. An increase of temperature would occasion an expansion and upraising of the strata, from which rents would result; and a reduction of temperature, causing a contraction of the rocks, would produce the same effects; in either case, the rents would be in nearly opposite directions, which might have been determined by the structure or joints of the rocks. Now, if the fissures resulting from an upraising of the strata, however produced, were to be partly filled with fragments or detritus of the rocks, or other mineral deposits, they would be wedged open as it were, and would not consequently, return to their original level, after the subsiding of the uplifting cause. In this way cavities might be formed at greater or less depths, and dislocations would probably follow, giving rise to some of the phenomena of faults.

The crust of the earth must, in some places at least, have been subject to great vicissitudes of temperature, if we may judge from the common occurrence of basaltic and trappean rocks, and even of volcanic matter, passing through, and resting upon, different strata.

It appears from the ratio in which the temperature increases in descending into our mines, that the heat may be about 212° Fahr. at something more than a mile below the surface; and

I fully believe, for many reasons, that the ratio of increase observed in mines is not so great as that of the earth itself, but that the *aggregate* effect of adventitious causes, operating in the former, has a tendency to reduce the temperature below its natural level.

It is highly probable that many of the veins penetrate to the depth of several miles: for taking them collectively, I apprehend, that their width is not sensibly diminished in our deepest mines, although some of the latter extend to between two and three hundred fathoms below the surface.* Great as this depth may appear to be, it is not a twelve thousandth part of the semi-diameter of the earth, or in about the same proportion as the thickness of common writing paper to a sphere of eight feet in diameter. The minute cracks which are sometimes observable in the varnish of a common globe may serve to give some notion of the size of the most considerable mineral veins in relation to the earth itself, although they may be many miles in depth as well as in length. However this may be, there can be little doubt that many of them penetrate into regions of great heat, and assuming that they have originated from fissures, they must have contained water even in their deepest parts, because the great pressure of the column, equal to one hundred and sixty atmospheres, even at the depth of a mile,—would, of course, have prevented its being converted into steam at the bottom of the fissures.

What then must have been the result of this state of things? It is evident that the heated water, at and near the lower parts of the fissures would ascend through the cooler water above, whilst the latter would descend, and occupy the place of the former, producing a circulation, more or less rapid, according to circumstances. Such circulation of heated water would naturally tend, in some measure, to wear away the sides of the fissures, and may have contributed to produce that degree of smoothness for which some parts of the walls of veins are often so remarkable. If any of the inclined fissures were sufficiently wide and open in some places, the ascending currents would principally act on the hanging walls, and the descending ones on the foot walls; and it remains to be ascertained by careful inspection, whether traces of the results of such action can be detected in the walls of any mineral veins. Stones more or less rounded, and apparently water-worn, are occasionally found in the form of conglomerates in mineral veins, but they are sufficiently rare to render it probable that the latter must have been mostly very narrow, or that the accumulation of matter in them might soon have become so considerable as to obstruct the easy circulation of the water.

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* The bottom of the Consolidated mines is now nearly 290 fathoms under the surface, and about 240 fathoms, I believe, below the sea level.

Let us suppose that in time, this circulation was almost stopped by mineral deposits, the temperature of the water at the lower parts of the fissures would increase, whilst that of the water above would decrease in a greater ratio. The inferior strata would, under these circumstances, have some tendency to expand, and the superior strata to contract; but if the former happened to be wedged open in any parts, by fragments of rocks and other substances, such expansion would tend to widen the fissures, whilst the contraction of the strata above, would, at the same time, augment this effect. Thus the rents might have admitted of the circulation of the water being renewed; and the same process might have been again and again repeated, until the fissures at length, became too much filled from the top to the bottom with mineral deposits, for such action to go forward with any perceptible effect.

I merely allude to this process incidentally, without intending to make it prominent, as it matters not to my present object by what means fissures were formed or enlarged from time to time. It is sufficient to believe that different causes may have operated with greater or less effect, and that they were fully adequate to accomplish the object.

It is, I believe, generally considered by miners, that the temperature of our copper lodes, is greater than that of the surrounding country, at equal depths ;* and they regard a copious jet of warm water into their workings as a favourable indication in a lode. Tin lodes are often rather inferior in this respect to those of copper, which is probably owing to their being generally harder and more compact than the latter.

I have, on different occasions, referred to granite being rather below killas in temperature ;† and W. J. Henwood has since determined, by numerous experiments, that the temperature of the former in our mining districts, is a few degrees below that of the latter. The difference in these cases, whatever it may be, I have long attributed to the facility afforded by lodes, as compared with the rocks; and of killas, as compared with granite, for the circulation of ascending and descending currents of water; and it is easy to perceive that the deeper lodes, and those parts most abounding with ore, may admit of such circulation more readily than where there is accumulation in them, of clay and finely divided mechanical deposits with quartz, &c.

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* From the mean of several observations made in mines, I have estimated this difference to be nearly three degrees. See Cornwall Geological Transactions, Vol. II., p. 29, year 1820.

† See Annals of Philosophy, 1822, Vol. IV., p. 447; and Philosophical Magazine for 1831, p. 99.

Assuming the hypothesis of a progressive enlargement of the greater part of the original fissures, it is not difficult to understand, that the formation of secondary ones, from time to time, would be the natural consequence of the rending force exerted, and of the tendency in the hanging sides of fissures sometimes to break off in vast masses from the adjoining country, thus forming cracks of greater or less extent. Hence, many lateral and branch veins may have originated, and likewise the masses of rock which so frequently occur in the veins, and which are termed by the Cornish miners, "*horses.*" But this subject will be again referred to in this paper.

Filling of Fissures.

It has already been stated, that mineral veins consist of substances resembling the enclosing rocks, and which are assumed to have been mechanically derived from them; and also of other substances which are so different from the contiguous rocks as plainly to indicate another origin; and such deposits, I conceive, may be referred to chemical, or electrical agency.

It is obvious that the splitting of rocks, and further opening of the fissures from time to time, would occasion fragments, and friable portions of the former to fall into the fissures, whilst the flowing of water into the latter, and its circulation within them, would tend to produce depositions of detrius of the rocks, clay, and other finely divided matter. These mechanical deposits would generally be accumulated in the largest proportions, in such parts of a given fissure, as had the greatest underlie; and miners find that lodes are usually less productive of ore in such situations, than in other parts which are more nearly vertical.

I have already explained my reasons for believing that the water at the bottom of some of the deep fissures, must have been at a very high temperature, and must consequently have caused a more or less active circulation in them, of ascending and descending currents. It is well known that the solvent properties of water, as well as of acids, alkalies, &c., are augmented by heat; and there is reason to believe that their power to dissolve matter, and to hold it in solution, increases in some ratio with the temperature.

It seems therefore to follow as a necessary consequence, that the hotter portions of water, &c., in the deeper parts of the fissures, would become charged with matter which it would deposit more and more as it ascended through them, and became gradually reduced in temperature; its partial evaporation when at the surface would also augment this tendency.

Amongst these deposits it is probable that silica or quartz, was most abundant, and that in many instances, it became intimately mixed with chlorite, and earthy matter, worn off from the sides of the fissures, by the action of the currents of hot water upon them.

The hot springs of Geyser, in Iceland, and Furnas, in St. Michael's, have accumulated silicious deposits around them to a great extent; and other thermal waters in various parts of the world, especially in India, and South America, are more or less charged with silicious matter, which they partly deposit on reaching the surface:—and as quartz is, perhaps, the most general and abundant substance in the majority of mineral veins, I think, there can be no hesitation in admitting that it may have been collected in the way now suggested. Other earthy substances of difficult solubility, without the aid of great heat, may also have been deposited in like manner, whilst the deposition of the more soluble compounds, and especially of the metallic ones, must be referred to other causes.

It is, moreover, highly probable, that a greater or less proportion of the contents of veins may have been derived, by infiltration, from the neighbouring country; and the circumstance of the rocks in the immediate vicinity of lodes being generally inferior in hardness to those at a distance from them, tends to strengthen this hypothesis. The frequent incorporation of quartz, chlorite, and other matter with lodes themselves, as well as with their sides, so as to obliterate any appearance of walls;—termed "*capels,*" seems to afford additional evidence in favour of infiltration. Such solutions, depositions, and changes, might be produced by a slow electro-chemical action, and the present hardness or softness of the sides of veins, and of the rocks adjoining, ought not certainly, to be referred to, as a criterion for what they formerly were in these respects. If it were needful, proofs might be adduced in abundance, of silicious matter having taken the place, and assumed the form of animal, vegetable, and mineral bodies, so as to compose masses, equal in hardness to quartz rock, or to the most compact "*capel*:" there can be no doubt then, that there exists in nature, a power of transference, such as I have alluded to.

I have found that pieces of compact granite and killas taken out of a mine, and without any apparent flaws in them, after having been placed in boiling water in which a small quantity of some salt was dissolved, were rendered conductors of voltaic electricity, although they possessed this property in a feeble degree in comparison with the liquid solution itself.

It is evident from this experiment, and from others made in mines, in which parallel lodes acted on each other through very large masses of rock, that rocks become conductors of electricity, especially at considerable depths, where the great pressure of the column of water, and the high temperature, combine to introduce moisture into them. If then, the rocks are thus rendered even feeble conductors of electricity, and contain different salts, they are necessarily, exciters of it also; that is, if contiguous, they become in opposite electrical states: the same remarks applies with still more force to moistened clay, because it contains more water.

I have proved by an examination of water taken from different mines, and from various parts of the same mine, that different varieties of saline solutions now exist in neighbouring strata. In some instances, the proportion of foreign matter in the water, was very small, whilst in others, it was considerable; but I have not yet tried any mine water, that would not produce very decided electrical action, when the native sulphuret and bi-sulphuret of copper were plunged into it, and the voltaic circuit completed. The very superior conducting power of the saline water in the fissures, in relation to the merely moistened rocks, would always tend to supersede the transfer of electricity, more or less, through the latter. The contact of large surfaces of rock, clay, &c., with water, differing in its saline contents from them, must also have been an efficient source of electrical excitement; and it should not be forgotten, that the circulation of the water would be liable to very frequent changes of velocity, in consequence of obstructions in the fissures, or their occasional enlargement, so that the contents, as well as the temperature of the water, would be subject to many modifications.

In many instances the rocks may have contained, as they now sometimes do, iron pyrites and other metallic substances, and in the deeper parts of the fissures, it can scarcely be doubted, that the sources of electrical excitement were greatly augmented and multiplied, not only on account of the high temperature of the water, which must have materially increased its conducting power, but likewise from the probable existence there, of some of the metals, in a pure, or nearly a pure state. If these points be conceded, it is difficult to assign limits to the extent of the development of electrical action.

Now it is a well known fact, that electric currents, by which I mean voltaic or thermo-electric currents, and magnetic bars, or needles, having freedom to take any position, have a tendency to arrange themselves at right angles to each other, and consequently as has been proved by experiment, an electric current transmitted through a very delicately poised conductor, will cause it to take a position at right angles to the magnetic meridian of the earth. If an electric current be directed from east to west through a metallic wire, a magnetic needle suspended *above* the wire, will have its marked end directed towards the north, but if the direction of the current be reversed, so will be that of the needle also, and the marked end will point toward the south. If the needle be suspended or poised *under* the wire, the order of its positions will be *inverted.*

Ampère has inferred from these—electro-magnetic properties, that the direction of the compass, or in other words, of the terrestrial magnetic meridian, is due to the circulation of currents of electricity round the globe from the east towards the west; and this opinion has, I believe, been very generally adopted. Such currents may, in many places, be more or less oblique with respect to the parallels of latitude. The aurora borealis, it is well known, sometimes appears in the form of an arc, the centre of which is intersected by the plane of the magnetic meridian, or nearly so; and it has been proved by observations, some of which were made by myself, that it has often a tendency to deflect the magnetic needle, and also to diminish the intensity of the earth's magnetism.

The aurora may, therefore, I think, be considered an exhibition of electric currents at a great height, which are connected with others nearly parallel to them, in the interior of the earth. Whether, however, we regard terrestrial magnetism as the effect or cause of the direction of electric currents, it cannot be doubted that these phenomena are in harmony with each other, and that if electricity existed under the surface, it would, if not counteracted by local circumstances, pass more readily from magnetic east to west, than in any other direction.* Hence, if fissures happened to have opposite horizontal bearings, and were equally filled with water charged with saline matter, the electric currents would be determined in preference, through such of them as most nearly approximated to the magnetic east and west points at the time.

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* If we suppose electric currents to circulate round the earth, from the east, towards the west, both above and below its surface, they would tend to deflect a magnetic needle in opposite directions, so that, in fact, terrestrial magnetism would be due to the excess of one over the other; and this hypothesis may possibly be found to be more consistent than any other with various magnetic phenomena, and particularly with the fact that the magnetic intensity has not been found to undergo any sensible change at considerable heights above, or depths beneath the surface.

Thus, for instance, if *e w*, fig. 38, Plate V., were at right angles to the magnetic meridian, and the fissures were formed in the direction *a b* or *c d*, the electricity would pass longitudinally through such fissures rather than through others nearly at right angles to them. The amount of the variation of the compass has been observed, in this country, slowly to oscillate through an arc of at least 36°, it being now nearly 25½°, to the westward of the true north at Falmouth; whereas, about 250 years ago, it was noticed in London to be 11° to the eastward of north, which gives a mean magnetic declination of 18° to the westward of the true meridian.

Taking it for granted, therefore, that the electric currents were chiefly confined to those fissures which most nearly corresponded with the magnetic east and west, they would act on the saline substances contained in the water, and gradually decompose them, the metal, or base, being determined towards the negative pole, or the electro-negative rock, and the acid, towards the electro-positive rock.

However slow this process might, at first, have been, the deposition of the metals would cause it to become more and more energetic. The metals and metalliferous deposits would, likewise, naturally react on each other, and give rise to new combinations and arrangements, till they arrived at a state of comparative equilibrium. This may be said to be very much the case in the lodes at present, as most of the ores which are capable of conducting electricity, very nearly approximate, to each other in the electrical scale; being more electro-negative than silver, and many of them as much so as platina; indeed, the grey oxide of manganese, and the loadstone are electro-negative in a still greater degree. Arsenical pyrites, iron pyrites, and copper pyrites, hold rather a high place in the scale, and are electro-negative with respect to purple copper, and galena, but more especially to the sulphuret or vitreous copper ore, which will produce a very decided action on a galvanometer, when connected in the voltaic circuit with copper or iron pyrites, &c.

All these ores, as well as some others, are of course, conductors of electricity; whereas the sulphurets of zinc and of silver are non-conductors. This is also the case in regard to other native sulphurets, and to most of the metallic combinations with acids and oxygen.*

It has already been stated, that the productiveness, and general contents of veins, seem materially to depend on the rocks which they traverse. Similar metalliferous deposits are, in some of our mines, principally found in one rock,—in granite, for example; and in other mines, in killas or elvan, although the same lodes may happen, in some instances, to traverse all these rocks, and very frequently copper and tin are found in *different* contiguous rocks: thus the former may chiefly occur in granite, and the latter in killas, or vice versâ.

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* See my paper in the Philosophical Transactions, in 1830, p. 399.

The character of the different beds of killas seems likewise to have had a decided influence on the deposition of the metals, and the miners lay great stress on the nature of the "*channels*" of ground traversed by the lodes, in their anticipations of their being productive or otherwise. The occurrence of oblique "*shoots*," and of "*pipes*" of ore, descending conformably to the underlie of the beds or laminæ of the killas, as mentioned by Captain Tregaskis, (page 17,) affords additional evidence of the connection between the strata and the contents of lodes. All the facts, moreover, seem to bear a remarkable analogy to some of the results of voltaic action. It is well known that by its means, the chemical affinities of bodies may be superseded, and even inverted; so it may, and it does seem to have happened, that metallic and earthy substances were determined towards certain rocks, and deposited on them, according to the relative electrical states of the latter. These states may have depended, either on the saline or metallic matter which the rocks contained, or on their positions and combinations with respect to other rocks, modified, more or less, by their relative temperatures at the time, as well as by the prevalent direction of the electric currents in their vicinity, and by numerous other accidental circumstances.

Becquerel has shown, that if a long slip of copper be put into a glass vessel partly filled with a solution of copper, and partly with water, or acidulated water, so carefully poured in as not to mix with the former, after some time, a precipitate of copper will appear on that extremity of the slip of metal which is in the solution. It is evident in this case, that the liquids were in opposite electrical states, and that the deposition of copper, was the result of voltaic action; for if the slip of copper had been put into a simple solution of that metal, the precipitation would not have taken place. The same philosopher has obtained, by means of weak and long continued electrical action, many of the metals, metallic sulphurets, and other metalliferous and earthy compounds, not a few of them beautifully crystallized, and precisely resembling those found in nature.

Crosse has, by means of large voltaic batteries excited by water only, also produced a great variety of metallic and earthy minerals, and amongst others, he has, I understand, obtained a crystal of quartz, nearly a quarter of an inch long.

When I learnt from this gentleman the results of his experiments, and persuaded him to communicate them to the Geological section at Bristol, I was, like himself, unacquainted with Becquerel's experiments, or at least, I had no idea of the method which he adopted.

After what has been stated, it might appear almost presumptuous in me to allude to any of my own experiments, did they not seem particularly to bear on, and to elucidate, some of the phenomena which are observed in the mines of Cornwall.

Following the arrangements which nature seemed to present, I placed different ores and metals in different saline solutions, separated merely by walls of clay in imitation of our flucan courses, and I completed the voltaic circuit between the ores, metals, &c., in the different cells so formed, by copper wire.

By these means, I found that yellow or bi-sulphuret of copper, in a solution of sulphate of copper in one cell, and sulphuret of copper, ("grey ore") in water or acidulated water in another cell; connected together by a copper wire, acted on each other, and after some weeks, the bi-sulphuret had a thin coating resembling the sulphuret. When zinc or iron, was substituted for the sulphuret of copper in the water cell, the bi-sulphuret of copper became coated with a considerable crust of the sulphuret, or rather it was changed into the latter to a greater or less depth, according to the duration of the experiment, in consequence of the abstraction of a portion of sulphur, and probably, also of some of the iron which it contained; and beautiful crystals of pure copper, were abundantly deposited upon it, and likewise, in some instances, red oxide of copper.

These experiments seem to bear on the fact of neither metallic, nor red oxide of copper, occurring in our mines in conjunction with yellow copper ore, but often with the sulphuret, or grey and black ore.

When a solution of sulphate of iron, was substituted for the sulphate of copper in the cell containing the copper ore, the latter appeared at first to have on its surface a deposition of iron, and this becoming oxidated, in time formed an incrustation, which resembled "*gossan.*" When the water in the other cell was acidulated by sulphuric acid, and a plate of zinc or iron, put into it, having a metallic connection with the electro-negative copper or iron ore in the solution of sulphate of iron, (or sulphate of zinc, as the case might be;) sulphuretted hydrogen was evolved from the latter, and sometimes in considerable abundance.

In the course of many of my experiments, I observed that the solution of sulphate of copper, for instance, became considerably elevated in the cell containing it, whilst the level of the water in the other cell was much depressed; indeed, in some instances, it was nearly all transferred. Hence, it may, perhaps, be concluded, that this modification of electrical agency, or *endosmose* and *exosmose* process, as it is termed, may operate under the surface of the earth, and that water may, by similar means, be raised to different levels, on opposite sides of flucan courses, &c. In some instances, there may happen to be a series of successive elevations, where circumstances are favourable:—the subject, at least, seems to deserve investigation in connection with the height of springs, &c.

I have observed that when the chloride of tin in solution, is placed in the voltaic circuit, part of the tin is deposited in a metallic state at the negative pole, and part at the positive one, in the state of a peroxide, such as it occurs in our mines. This experiment may serve to explain why tin is found contiguous to, and intermixed with copper ore, and likewise separated from it, in other parts of the same lodes, or in other lodes situated near, or perhaps crossed by the copper lodes.

It appears that copper, iron, zinc, and other metals in solution, are, under ordinary circumstances, determined toward the negative pole; and the fact of "*gossan*" being found in copper lodes, and not in those of tin, is quite in conformity with the ascertained properties of the respective metals.* Some metals, however, like tin, assume the properties of acids, if combined with oxygen, and consequently, when in this state, tend towards the electro-positive pole. It has been remarked that tin lodes are often harder than those of copper, and this is perhaps, owing to the determination, by electrical agency, of a portion of silica to the electro-positive pole.

Having endeavoured to show that electric currents must have existed under the earth's surface;—that their tendency, on electro-magnetic principles, must have been, *cœteris paribus*, to pass longitudinally through those fissures which most nearly coincided with the magnetic east and west points:—

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* I have already noticed that many large iron lodes are nearly coincident with the magnetic meridian: this direction, when regarded in connection with the magnetic properties of iron, is interesting.

and that in proportion as they decomposed the earthy and metallic salts held in solution or otherwise existing, they would determine their constituent parts toward opposite poles, or rocks in opposite states. I shall next refer to some circumstances, which may have interfered with the full and undisturbed operation of these laws.

These may partly have been of a mechanical nature.—Clay, or other earthy accumulations in the fissures may, by forming intermediate poles, have arrested the transfer of the metal in given directions, and thus produced depositions of ore in various parts of the fissures, short of the most decidedly electro-negative rock. Suppose, for instance, that the general tendency of the electric currents at any place, were towards the granite *g g′*, fig 35, Plate V, but that the contraction of the lode or fissure, *e w* at *d*, and the accumulation of earthy matter there, checked the transmission of the metals so much, as to produce a deposition of them in the wider part of the fissure, the result might be a bunch of ore, at *f*, connected with the small vein or string at *d*.

There are other causes of an electrical or chemical nature, which may, in some instances, have had a powerful influence in determining the relative positions of mineral deposits.

It is well known that tin in solution has a strong affinity for oxygen, and will rapidly attract it from the atmosphere, forming with it an insoluble peroxide; and hence, may partly have arisen the circumstance of this metal, being commonly found near the surface, on the back of copper lodes, and more or less intermixed with "gossan." This property of tin seems, moreover, to account for its occurring more dispersed than copper, and upon the whole, rather less conformable in its positions to general rules.

Solutions of iron have also a tendency to absorb oxygen from the atmosphere, and to form an insoluble oxide, (iron ochre;) but it is very inferior to tin in this respect; and this circumstance may be another reason why "gossan" is not found on the backs of tin lodes, since the superior affinity of the latter metal for oxygen, when in solution, would naturally interfere with the absorption of this gas, by any iron dissolved with it.

It is to be inferred, from what I have before stated, that sulphuretted hydrogen might have been abundantly generated by the action of electricity on yellow copper ore, or even on iron pyrites; and it is well known that this gas will immediately throw down the metals, except iron, and a few others, in the state of sulphurets, in the order of their respective affinities, and tin would be amongst the first to be so precipitated.

It may, perhaps, be imagined, that this re-agent would have precipitated the metals, more or less, throughout the whole extent of a given fissure; but it must be remembered, that its action might have been very much concentrated, near the parts at which it was generated, if there were a sufficient supply of metallic salts in solution in the same vicinity, to absorb it. Moreover, it cannot be doubted, as I have before remarked, that the metallic substances must have reacted on each other: thus a considerable accumulation of an electro-positive metal, such as zinc or iron, at an electro-negative rock, might, for a time at least, have changed, or reversed the direction of the electric currents.

The pseudomorphous crystals of various kinds, and especially of quartz, which are of such common occurrence in our mines, afford decided evidence of reaction. We find, for example, the yellow sulphuret of copper in the form of crystals of carbonate of iron, which it must have gradually displaced; oxide of tin, in the form of crystals of felspar; and the sulphuret of lead in six sided prisms,—termed blue lead,—having superseded the phosphate of that metal. Fig. 52, Plate VII, represents a crystal in my possession, the shaded part of which is blue lead, and the unshaded part translucent phosphate of lead, not yet decomposed.

The appearance of another crystal in my cabinet, is shown by fig. 53;—the light part represents pseudo-hornstone, projecting through the centre of a crystal of octohedral blue fluor, part of which still remains, and is represented by the shaded part.

Fig. 54, represents a group of large quartz crystals, with crystals of iron pyrites on one side, and of copper pyrites on the other, with a line of separation between them. This curious arrangement seems to be due to electricity, and a great many of these crystals were found some time ago in the Consolidated mines.

The enumeration of results of secondary action in veins, might be greatly multiplied, proving that substances not soluble under ordinary circumstances, have been transferred into, and taken the forms of other bodies; to say nothing of the evidences of secondary action afforded by the numerous metalliferous deposits, which are found in veins, situated in fossiliferous strata, and precisely resemble many of those which occur in the mines of Cornwall. Can it then be doubted that many of the phenomena observable in our mines, are caused by similar actions and reactions;—by such an agent, in fact, as electricity is known to be, seeing that time, and power, on the most extended scale, have not been wanting for their production?

It has been asked, why, if metallic deposits were produced by electricity, they are found in fissures or veins, rather than in the unbroken parts of the rocks? The much more easy transmission of electricity through the fissures is one reason, which has been already urged, and the continual change of the water and salts in them by circulation, may be referred to as another; since it is obvious that the moisture absorbed by the rocks, must always have been in a comparatively stagnant state.

Many of the cross veins, as has been mentioned, abound with quartz of a fibrous or radiated texture. Fig. 50, represents a fragment of each quartz. At *a*, the crystals of quartz point toward each other; *b* and *d*, seem to be laminæ of killas, or chlorite and quartz intermixed; at *c*, the division between the quartz appears principally to consist of iron ochre; and at *e* there is a surface of killas and quartz like the outer walls of the including vein.

The quartz may have been chiefly derived from the deeper parts of the fissures, as in the case of lodes, and partly, perhaps, from the neighbouring rocks, by means of infiltration. Its striated arrangement may be owing to the sides of a given fissure having been in opposite electrical states, such as are acquired by the contact of conductors, or by the intervention of a liquid between them; i. e. the electricity, instead of having passed longitudinally through the fissure, may have crossed it from one wall to the other. This inductive state, or tendency in electricity, to traverse a vein at right angles to its direction, may, under given circumstances, be produced even in lodes, in consequence of the action of parallel lodes on each other, as will presently be more particularly noticed.

Does not the remarkable arrangement which has been alluded to, arising apparently from position, rather indicate the existence of some general laws, to which even the directions of the joints of rocks may be referred? It has been proved, that substances which are considered the most insoluble, may be acted upon, and re-arranged by feeble, and long continued voltaic action; so it is possible, that an elementary arrangement may have taken place, even in moist *mechanical* deposits, and have imparted to many rocks, the characters which they possess.

The clay and disintegrated portions of the enclosing rocks which exist in some cross courses, and in flucans and slides, are evidently of mechanical origin.

Intersections and Dislocations of Veins.

Intersections have been supposed to afford certain evidence of the relative ages of veins; the *intersected* vein having been assumed to have had an anterior origin to the *intersecting* one. How far this distinction may be true, generally speaking, I will not pretend to determine; but I think it can be shown, that the mere fact of intersection, ought not, apart from other considerations, to be taken as evidence of the relative ages of veins. On the contrary, I believe, that in many instances, intersecting veins had, at least, as early an origin as those which they traverse: how otherwise are we to account for many very complicated intersections which occur in our mines? To illustrate this point, let us suppose *e w* and *n s*, fig. 33, Plate V, to represent small rents or fissures, co-existing in opposite directions, and that *e w* became gradually filled with mechanical, chemical, and electrical deposits, and *n w*, with mechanical deposits only, or mixed with quartz. It is evident that the mechanical deposits, and even the quartz derived from the circulation of the water, would have a tendency to accumulate at the points of intersection more rapidly than the metals, &c., would be deposited there, by the agency of electricity. Suppose both veins simultaneously to have undergone a subsequent enlargement as represented by fig. 34, their contents would be completely separated at *a*, and the opening would be immediately filled, in part, by the debris of the rocks and veins; whilst the descent and circulation of the water, would tend to produce an early deposition of clay and finely divided matter, and thus the intersection of the lode, *e w*, by the more mechanical cross vein, *n s*, would still be apparent.

Hence it may be concluded that when veins, crossing each other, become expanded, the softer and more mechanical vein, will have a tendency to intersect that which is more crystalline and firm in its composition. Thus we find that tin lodes, which are usually harder than those of copper, are intersected by the latter; whilst both are, in most instances, intersected by cross courses, flucans, and slides. It seems, however, that when copper lodes abound with clay, they often intersect the cross courses. On this principle, we are enabled to explain the fact of a vein, *a*, intersecting another *b*, at one level, and being itself intersected by *b*, at another level.

The dislocations, or "heaves" of lodes, may have been sudden, or increased at intervals. If a given heave occurred at, or soon after the first formation of the cross fissure, the laminæ or included veins, would, by their regularity and parellalism, probably indicate successive periods of expansion, as shown in *n s* fig. 36, Plate V, and fig. 50, Plate VII, but if a dislocation took place when the cross vein had attained its full size, or nearly so, its contents would be disturbed and the laminæ confused and irregular, if not obliterated; see *n s*, fig. 37.

At the time of the dislocations of any given lodes, it will readily be admitted, that the violent fracture and disturbance of the rocks, might have produced numerous minor rents, and even very minute cracks, near the dislocated parts, which, when filled with metallic or earthy deposits, would constitute small veins and strings, which *c d* and *e*, fig. 36, may help to illustrate. Veins so formed, are sometimes so diminutive as to serve for hand specimens, showing intersections in miniature. It has been remarked that such small veins of ore connected with a dislocated portion of a lode, on one side of a cross course, have not, usually, any corresponding veins connected with the part of the lode on the other side of it; in fact, they afford evidence of not having been formed prior to the dislocation. The same observation applies to the frequent accumulation of ore in lodes close to cross courses, either on one side only, or on both sides of the latter. In such cases, it can hardly be doubted, that the ore must have been deposited after the intersection or heave happened, and this hypothesis is corroborated by the frequent occurrence of some ore, in the cross courses, near the intersections. If, then, much of the ore were deposited after the formation of the cross fissures, how does it happen to be accumulated in E. and W. veins, rather than in N. and S. ones, if it be not referred to electro-magnetic agency?

It is obvious, that the progressive openings and dislocations of fissures, might have given rise to other secondary fissures, in consequence of the rending force exerted, or the subsidence of the hanging sides of the former, when their supports in the fissures were disturbed. Thus if the hanging side of the fissure or lode *m y*, fig. 36, subsided, the secondary vein or lode, *f g*, might have been the ultimate result; and the breaking off of the hanging side of the cross course, *n s*, fig. 36, might, in like manner, have produced the vein *h k i*, and the other smaller veins nearly parallel to it. The section represented by fig. 49, Plate VII, will help further to explain the probable origin of such subsequent fissures, or veins; suppose the whole mass *a b c* to have given way, with the included portion of a tin vein *l o*, the latter will exhibit opposite heaves or throws, upwards and downwards, as at *l o n*.

It may sometimes happen, that an apparent break in a lode, may have arisen from a fissure, in traversing another horizontally, having taken a zigzag direction, as represented in fig. 6. In this case, the lode *e w*, is supposed to have originally passed a little to the right in the cross fissure *n s*, and then to have resumed its previous course. This case is only to be distinguished from a real dislocation, by its not being found to accord with other shifts, or by there being none in its immediate neighbourhood.

The appearance of a dislocation may have been also produced merely by the enlargement of an intersecting lode: thus let *a b*, fig. 47, Plate VI, represent the section of a copper lode, and *c d*, that of a tin lode, the latter would seem to be dislocated as at *e f*, by the opening of *a b*, in the direction of the dotted lines, after *c d* was formed. The enlargement of the fissure would also be more considerable in its vertical, than in its inclined parts: this will be illustrated by comparing *a* and *b*, with *g*, fig. 47; commonly, however, I believe it will be found, that the hanging walls of veins have subsided more or less, and exaggerated these phenomena.

Fig. 48 may represent a dislocation caused by a diagonal motion of a mass of rock, as it were, on a pivot or axis. Let the dotted lines *a b*, represent the section of a gossan or copper lode, and *c d*, of a tin lode, in their original positions; and suppose them to have fallen over to the positions *a′ b′* and *c′ d′*. The slipping of the walls *a b* against each other, would destroy their parallelism, and cause a considerable dislocation of the tin lode as at *e f*.

It will generally be easy to ascertain the direction of the motion which has produced any given heaves, if other veins, differing in their dip, occur in the same neighbourhood.

Thus let fig. 39, Plate VI, be a ground plan, and fig. 40, a section of three lodes, one of which is vertical, and the other two dipping towards each other; and let *n s* represent a cross course, by which they have been dislocated. It is easy to perceive, that the phenomena represented in the diagram, can only be accounted for, by a vertical motion, as shown by the dotted lines, fig. 40. In like manner, figs. 41, and 42, clearly indicate a diagonal motion on one side of the cross course, in the direction of one of the lodes; and figs. 43 and 44, a horizontal motion, all the lodes having been equally heaved. Figs. 45 and 46, illustrate other varieties of heaves, the single lines represent the horizontal bearings of different cross courses, and the double lines those of lodes which have been shifted by the former. Now, as the lodes dip in the direction of the arrows, a small subsidence of *a*, fig. 45, and a much greater one of *w*, would produce the effects observed; and so would, of course, an elevation of *a*, and of *e*. In fig. 46, a depression of *e*, and a more considerable one of *b*, or an elevation of *w* and *e*, would account for the relative situations of the dislocated parts of the lodes.

Other explanations might evidently be given, but it is, perhaps, unnecessary to enlarge on the subject. It may, I apprehend, be remarked, that the dislocations of the veins in Cornwall do not, generally speaking, indicate that these phenomena are owing to vertical, so much as to lateral, or sliding motion, having a greater, or less degree of obliquity.

The extent of heaves often seems to vary considerably at different depths. This may sometimes be owing to a cross vein having split into branches in descending, as in a case at Huel Jewel, mentioned to me by Captain Jeffery, or changes in the underlie of the lodes may have produced only an apparent difference in this respect.

The occurrence of large masses of rock in veins which are sometimes quite isolated from the strata or country, cannot well be explained on mechanical principles, except on the hypothesis of the progressive opening and filling of the fissures. Let us suppose the vein, *e f d*, fig. 49, to have been small at first, and to have been afterwards opened by some rending force, in consequence of which, the mass *m*, became detached from the hanging side of the fissure, and rested on the mineral matter previously deposited in the vein: a fissure between *m*, and the hanging wall, would be the result; and when this became filled with mineral matter, the mass *m*, would constitute what in Cornwall, is termed a "*horse*," and in the north of England, a "*rider*." These "horses" are frequently penetrated in different directions by small veins of quartz, ore, &c, the cracks from which they originated, having probably been caused by the movements and pressure of the superincumbent rocks.

It has been remarked that the mechanical deposits would naturally accumulate in the inclined, rather than in the more vertical parts of fissures; which, as well as the inferior width of the former, will sufficiently account for their being generally less productive of ore. Such mechanical deposits on the foot wall, which may become in time, almost incorporated with it, and the giving way from time to time of portions of both walls, but more especially of the hanging one, may, amongst other causes, well explain why the indications of them are often very undefined, or entirely effaced.

The small veins, or laminæ of different metalliferous, or earthy deposits, included within the walls of a principal vein, are frequently parallel, but sometimes oblique, with respect to the sides of the vein. The former position, as I have before remarked, affords decisive evidence in favour of a progressive opening and filling of the fissures, and the latter, or oblique position of the included veins, strengthens the hypothesis, as it may be attributed to their having adhered to one wall, in some parts, and to the other wall, in other parts of a fissure, during its expansion; or it may, in some instances, have been caused by cracks or rents across the veins, which were afterwards filled up by mineral matter or clay; see fig. 47, Plate VI. At *h*, it will be noticed that the indications of included veins, are very indistinct, owing to the effect of disturbance under great pressure, and to the metals, reacting on each other and entering into combinations* after their deposition: whereas at *i*, parallel and oblique veins are represented in the lode.

Having thus given my views as to the origin of some of the secondary and branch fissures, I shall next endeavour to account for the deposition of ore in them, even when they happen to be almost at right angles to the prevalent bearings of the lodes.

I have found by experiment, that when the ore contained in two parallel copper lodes are connected by wires, the electricity transmitted through the latter, nearly at right angles to the direction of the lodes, is often very energetic. If the fissure *h k*, fig. 36, contained water with salts in solution, it would evidently, like the wire, conduct the electricity between *w* and *y*; and the latter would tend to decompose the salts, and deposit the bases at the negative pole or extremity of the fissure: and the deposition would proceed onward in the fissure, till it reached the other extremity, provided a sufficient supply of salts were brought under the electric influence by circulation, or otherwise. This operation might, however, be checked by the accumulation of mechanical deposits in the fissure, or by silica, or other substances, some of which might have been determined towards the electro-positive pole.

On the same principles, minute veins or branches, and "*droppers,*"† which ramify in different directions from lodes, may have been filled with ore, &c. These are sometimes termed "*feeders,*" because they are often connected with masses of ore, but they ought, perhaps, rather to be viewed as the effects, than the causes of the productiveness of lodes in their vicinity.

The small veins or "*leaders*" of ore, which are often observed in cross courses, between dislocated parts of lodes; (see *a* and *b*, fig. 36, Plate V.) may likewise be referred to the same kind of action between the divided portions of the lodes; and when the ore in the cross courses extends beyond these boundaries, as it sometimes does, it may have been so deposited by the influence of more distant lodes; or in consequence of the acuteness of the angles of intersection which has caused the cross courses to partake, in some degree, of the nature of branches of the lodes.

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* It is evident that any of the metals found in our mines, which might, at first, have been deposited in a pure state, would quickly enter into new combinations, when reacted upon by copper and iron pyrites. The more electro-positive metals, such as zinc, tin, iron, &c., would of course, be exposed to the influence of such reaction before copper.

† "*Droppers*" are shown at *r*, *s*, and *d*. Fig. 49, Plate VII.

The arrangement of some of the metallic veins, with respect to each other, is often very remarkable; thus it happens, that in those parts of Cornwall where copper and tin are abundant, lead, and some other metals, when they occur, are found principally in cross courses; whereas, I apprehend that in some other mining districts where lead greatly predominates over copper, the former is in east and west veins, and the latter in cross veins; but I am not prepared to speak positively on this point, or to lay it down as a rule. If it should be confirmed by further inquiry, the fact may perhaps, be attributed to the well known tendency which is observable in portions of any given metal, to cohere or become aggregated with other portions of the same metal previously deposited; and to the influence of electrical action, between parallel lodes of other metals, which happen to occur in any place, in the greatest abundance.

It has been observed, that when two lodes of the same denomination, unite at acute angles, either in their strike or dip, they generally continue together for some distance, and are enriched whilst in conjunction. This result is quite consistent with the increased electrical action naturally produced by the union of two nearly parallel currents. When, however, lodes cross each other in their underlie at very considerable angles, they are, it seems, usually impoverished at and near the points of intersection. Now, if the electric currents were in opposite directions, in fissures so circumstanced, this ought to be the result, as they would tend to neutralise each other at the parts of intersection; and it is remarkable, that even now, the electric currents seem, in most instances, to proceed from the eastward in lodes having a north underlie, and from the westward, in others, having a south underlie.* W. Henwood has recently published in "Sturgeon's Annals of Electricity," a summary of numerous observations made by him, on the electricity of lodes, which, generally speaking, show this tendency, although not so constantly as those which had previously come under my notice; but it is scarcely to be supposed that local causes should not often modify the direction and energy of such currents.

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* See Philosophical Transactions, 1830, p. 401.

I am decidedly of the opinion, that the electro-magnetic phenomena which have hitherto been detected in mines, were caused by voltaic, rather than by thermo-electric action. To produce the former, we separate good conductors,—different metals, for instance, by a less perfect one,—such as saline water, moistened clay, &c.; but to generate the latter, we employ a good conductor, such as a piece of metal, uninterrupted by an imperfect one; and heat one extremity of it. Now, it has been ascertained, that the greatest electrical effect was observed in mines when masses of ore, separated by clay, and other imperfect conductors, or parallel lodes, were connected by wire; and if, in a few instances, currents were detected when different parts of apparently continuous masses of ore were so connected, it is highly probable that they were more or less divided by joints, which however minute, would tend to excite voltaic action, and to produce currents through the wire, as being the superior conductor. Nor does the fact of the deeper and warmer parts of copper and lead lodes, having, in most instances, been found electro-negative, seem to accord with the thermo-electric properties of the sulphurets of these metals, which are rendered electro-positive by heat. Upon the whole, it is probable, from the comparatively small difference of temperature between the extremities of any perfectly continuous mass of ore in our lodes, that the influence of thermo-electricity, is too feeble to be easily detected by ordinary means; or at any rate, it must be considered as holding a very secondary place indeed, in producing the electro-magnetic phenomena which have been observed in mines.

Recapitulation.

In the theoretical part of this paper, I have, amongst other things, endeavoured to show:—

1. That admitting the origin of mineral veins to have been derived from fissures in the earth, there is reason to believe that the latter may have been produced by different causes, and at various intervals; also that many of them have been enlarged from time to time.

2. That the accumulation of mineral deposits in such fissures has been likewise progressive; and that the evidences afforded by the resemblance of the vein stones to the several enclosing rocks, and the arrangements and subdivisions of the contents of veins, are decidedly in favour of both these conclusions; independently of other arguments, based on mechanical principles.

3. That the phenomena of veins seem to indicate that many of the fissures penetrated to a great depth, and into regions

of very high temperature, and that consequently the water which they contained must have circulated upwards and downwards with greater or less rapidity.

4. That since the solvent power of water seems to increase in some ratio to the augmentation of its temperature, it is obvious that it would tend to dissolve some substances at a great depth, which it would deposit, more or less, in the course of its ascent through cooler portions of water; and also in consequence of its partial evaporation on reaching the surface.

5. That a part of the earthy contents of veins, and more especially silica or quartz, was apparently accumulated in this manner, and usually combined, more or less, with matter otherwise deposited.

6. That rocks, clay, &c., containing different saline solutions, and metalliferous substances, in contact with water, charged in many instances, with other salts, were calculated to produce electrical action; and that this action was probably much increased by the circulation of the water, and differences of temperature; but more particularly by the existence of compressed and heated water, metallic bodies, &c., at or near the bottom of the fissures.

7. That since the water in the fissures containing metallic, or earthy salts, was a conductor of electricity, especially when heated, and in a very superior degree to the rocks themselves; it is evident, that in conformity with the laws of electro-magnetism, the currents of electricity would, if not otherwise controlled, pass towards the west, through such fissures as were most nearly at right angles to the magnetic meridian at the time.

8. That the more soluble, metallic, and earthy salts may have been decomposed by the agency of such electric-currents, and the bases been thereby determined, in most instances, towards the electro-negative pole or rock; that tin, however, under these circumstances, is only partly deposited at the electro-negative pole, and partly at the electro-positive pole, in the state of a peroxide; and that these properties of this metal seem to bear on its positions in the lodes, with regard to copper, being sometimes found with it, and sometimes distinctly separated from it.

9. That the position of one rock with respect to another, or to a series of other rocks, may, as well as their relative saline or metallic contents, temperature, &c., have had a decided influence on the deposition of minerals on them by electrical agency, so that a given rock may have been *electro-positive* in one situation, and *electro-negative* in another, in

regard to other neighbouring rocks, as this is quite consistent with voltaic phenomena.

10. That the evolution of sulphuretted hydrogen, and the tendency of some metals, when in solution, to absorb oxygen, and become insoluble, may, in many instances, have interfered with the regular arrangement of the metals, such as electricity would have effected; and that hence, many anomalies may have arisen, especially in relation to tin.

11. That the electrical reaction of the different metalliferous bodies, and of masses of ore on each other, after their deposition in the fissures, may have corrected such anomalies in some instances, and that they may have given rise to them in others, by changing the direction of the electric currents, and thus modifying the relative positions of the deposits; and that the pseudomorphous crystals of various descriptions, as well as other phenomena observable in mines, fully prove that some such secondary action must have taken place.

12. That cross veins may have been filled mechanically, or by the deposition of silica from a state of solution, or by both these means; and that the striated and radiated structure of the quartz may be owing to the tendency of electricity, under ordinary circumstances, to pass transversely, rather than longitudinally, through N. and S. veins.

13. That assuming the proofs of the progressive opening and filling of lodes and cross veins to be admitted, it seems to follow that many intersections may have been caused by the more ready accumulation of clay, and other mechanical matter, and even of silica from its solution, than of the more slowly formed metalliferous or crystalline deposits.

14. That the frequent occurrence of a mass of ore in that part of a lode which is intersected by a cross vein; and also of small branches of ore from a dislocated part of a lode on one side of a cross vein, without there being corresponding veins near the other part of the lode, on the opposite side of the cross vein, afford strong evidence of the deposition of the ore in such cases, after the intersection took place; and that it was accumulated in the E. and W. vein, rather than in the N. and S. one, by the influence of electro-magnetism.

15. That the small veins of copper and tin ore which are often found in cross veins between the dislocated parts of lodes, and the frequent occurrence of more considerable, and yet, for the most part, very limited quantities of these ores in the former, in the immediate vicinity of intersections, are additional arguments in favour of the operation of the same definite agency.

16. That the secondary fissures, resulting from the cracking off of larger or smaller masses of the hanging sides of

veins, may have been partly filled, in many instances, by the electric action of different portions of ore on each other; and that secondary lodes may have been formed at right angles to parallel E. and W. lodes, in consequence of the reciprocal action of the latter.

17. That many other phenomena of mineral veins, including those of a mechanical character, such as the occurrence of horses, heaves, &c., appear to be capable of satisfactory explanation on the principles which have been laid down.

Conclusion.

Imperfect and limited as is our acquaintance with mineral veins, enough is known to excite our admiration of the order and fitness which prevail amongst them.

We observe that many of the most useful metals are the most abundant:—and the fact that they are generally confined to certain veins, and to certain portions of them only, is perhaps, of greater import, than we might at first suppose:—for had they been disseminated in the strata, or even dispersed throughout all mineral veins, the labour required to obtain them, would have rendered them practically useless:—or had they, on the other hand, been much more concentrated, their rapid exhaustion might entail incalculable injury on future generations.

Again, we remark that few metals are found in a native state, and those very sparingly;—were it otherwise, there is good reason to believe that their electric action and reaction would have been so energetic, that some of the electro-positive metals could not have been permanently deposited. Had the metals generally existed in combination with oxygen or acids, their electric action would have been reduced to a minimum quantity; in which case, many metallic and other solutions, might, from being but partially decomposed, have found their way to the surface, and impregnated the springs with their deleterious qualities. Sulphur appears to be the only component which enables metals to effect all the required conditions, and this proves to be the combination in which they most frequently occur. It has already been mentioned that such of the metallic sulphurets as conduct electricity, are highly electro-negative, and that their reciprocal action, in most instances, is so nearly in equilibrio, as to prevent considerable changes; nevertheless, they seem to possess sufficient electric activity to act upon other bodies, and to decompose saline solutions which may be exposed to their influence:—and who knows how important such electrical filtration of the ascending water may be, to organic existence at the surface of the earth?

Some of the cross courses appear to be channels for conducting the water between different lodes; and the flucan veins, by occasionally intercepting it, tend to prevent the too great drainage of the land which would otherwise attend mining operations, whilst at the same time, they enable the miner to prosecute his labours under-ground, to a much greater depth than would else be practicable.

Considering the nature and arrangement of mineral veins under the surface, it can scarcely be doubted that the *endosmose* and *exosmose* process must prevail more or less, within the earth, and tend to cause differences in the water level on the opposite sides of flucan courses. This influence, as has been before remarked, may be sometimes exerted in the same direction, through a series of parallel clay veins, so as to produce successive elevations of the water, and thus affect the relative heights of springs. There are good grounds for believing that some of the most curious phenomena connected with respiration, and various animal and vegetable secretions depend on the same process, which is itself apparently a result of electrical agency.*

This at least, is certain, that the action of electricity is not confined to the surface of the earth; and it is more than probable that it is inherent in all matter in some modified form; so that, should the Hand that produced it, suspend its operation but for one moment, animal and vegetable life would be universally extinguished.

I have already alluded to the property which water possesses, in common with all other fluids, to ascend when heated, and to the influence of this property, in conjunction with

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* Does not the counteracting effect of external irritation in inflammatory cases, appear to be a result of the same principle, which causes an electro-positive metal to preserve an electro-negative one, when they are together exposed to the action of diluted acid? If this analogy be well founded, it seems to follow, that in order to insure the most beneficial effect, the external irritation should be kept up without intermission.

In the Philosophical Magazine, vol. V. p. 7, I have suggested that the electric *elements* (if I may use the term,) may, like electrified bodies, possess opposite poles, and that the *inversion* of these poles, to a greater or less extent, according to the relative conditions of the contiguous substances, may simply and satisfactorily account for *induction*, and other electrical phenomena. I can in no other way conceive, how the changes in the electrical condition of different bodies when brought into contact, and the perfect balance of forces thus induced, are to be explained.—Why for instance, is silver when placed in diluted acid with copper, protected, at the

the high temperature of the interior of the earth in the filling of fissures with mineral deposits. Thermal springs seem to be also a result of the same causes, and it is unnecessary to enlarge on their uses to mankind. Were it not for this law of fluids, the great ocean-currents which circulate between the polar and equatorial regions, tending to equalize the temperature of the earth's surface, would cease to flow, and the atmosphere would be comparatively stagnant and unfit for respiration.

Evidences might be multiplied, almost without limitation, to show the perfect adaptation of simple general laws to every possible case in the whole circle of creation. We can, however, detect their existence only by their effects, for our perceptions are far too limited, and our comprehensions too feeble to understand the elementary constitution even of the simplest form of matter. There is, nevertheless, in all nature, a harmony of parts, and a consistency of operation, calculated to excite our reverence and gratitude towards her Almighty Author, whose infinite fore-knowledge and goodness thus forcibly manifest themselves, in the perfect adaptation of physical laws in every existing circumstance.

expense of the latter, which, under such circumstances, is acted upon with increased energy?—and why, when iron is substituted for the silver, is the previously *electro-positive* copper, rendered *electro-negative*, and protected by the iron;—and this again by zinc?—In such cases, an actual transference of an electric fluid can scarcely be imagined. The fact of many compound bodies, which are *non-conductors* of electricity when *solid*, becoming *conductors*, when *fluid*, appears to be in accordance with the hypothesis alluded to. In the former case, the poles may, perhaps, be retained with a double force by the components, so as to prevent their inversion, whilst in the latter, the fluid particles may allow a degree of freedom of motion to the poles, and be turned with them.

When I referred to the circumstance of fluids being capable of acting as voltaic poles, pages 32 and 36, I omitted to mention an experiment which I showed to some of my friends early in the autumn. On immersing a bladder containing acidulated water, and zinc, in a solution of sulphate of copper, the copper was deposited in a pure state on the surface of the bladder: hence, I conceive, that the sulphate of copper must have acted as an electric pole, and been electro-negative with respect to the zinc from which it was separated by the bladder. The fluids *within*, and *without* the bladder, were observed, in the course of some weeks, to stand at very different levels, the former being five or six inches above the latter, although the bladder was open at the top.

XXXIV. *On the Causes of the Tornado, or Water Spout.* *By* R. Hare, M. D. &c., &c.

In July last, I visited the scene of the tornado, which had in the previous month, produced so much damage in and near New Brunswick, New Jersey, and heard it described by various witnesses, and have likewise been edified by the observations made respecting its effects by professors Henry, Torrey, Johnson and other sagacious and learned observers, and especially those of my friends, professor A. D. Bache, and Mr. Epsy. Probably in no other instance have the effects of a tornado been so faithfully and skilfully traced, ascertained and registered. Professor Bache regularly surveyed the path of the devastating agent, and ascertained the bearings of the various bodies prostrated by it, so as to make several accurate plots.* From an examination of these, the proximate causes of the changes effected, are those of a vertical current at the centre or axis of the tornado, and of a horizontal conflux of the air towards that axis from the surrounding space. Some trees appear to have been thrown down on the approach of the hiatus, both directly in front of it and on either side; some fell at right angles, others obliquely to the path. Hence they were found to have a great variety of bearings, but always pointing towards the path. The time of their falling, and consequently the direction agreeably to the observations of professor Bache, appear to have been determined not only by the extent of the force to which they were exposed, but likewise by the strength of their roots, or the degree of protection afforded them by other bodies, trees or houses for instance. On these accounts, neighbouring trees, falling at different times, had different bearings; but that they all fell towards the point occupied by the axis of the tornado at the time of their overthrow, appears to be consistent with the facts. In one instance, both professor Bache and Mr. Epsy observed that the post of a frame building, being dislodged from the stone on which it rested, was first moved towards the path of the tornado in one direction about eighteen inches, marking its course by a furrow in the ground, and afterwards moved in another direction, nearly at right angles to the former, leaving a similar indication of the course in which it had moved. Intermediately between the time when the tornado bore in those directions, the frame was protected by a house.

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* I hope that these plots will appear in this volume.

While the phenomena above described sufficiently indicate the existence of a horizontal conflux of the air, that of a vertical force was demonstrated by the transportation of the debris of the houses and trees, as well as lighter bodies, to a great distance. A lady's reticule was carried seven miles from New Brunswick, and a letter twenty miles. The piece of timber, technically called the plate, on which the rafters of the roof of a meetinghouse in New Brunswick rested, was carried nearly a quarter of a mile, and lodged in some trees beyond the Raritan. The fields, on the other side of that river, were strewed with shingles torn from the houses in the town.

After maturely considering all the facts, I am led to suggest that a tornado is the effect of an electrified current of air, superseding the more usual means of discharge between the earth and clouds in those sparks or flashes which are called lightning. I conceive that the inevitable effect of such a current would be to counteract within its sphere the pressure of the atmosphere, and thus enable this fluid, in obedience to its elasticity, to rush into the rarer medium above.

It will, I believe, be admitted, that whenever there is sufficient electricity generated to afford a succession of sparks, the quantity must be sufficient, under favourable circumstances, to be productive of an electrical current; and that light bodies, lying upon one of the electrified surfaces, may be attracted more or less by the other.

The phenomena of the rise and fall of electrified pith balls, called electrical hail, sufficiently justify this last mentioned statement; while the continuous stream is illustrated by the electrical brush, or the blast of air produced by a highly electrified point.

It will also be conceded, that thunder and lightning are caused by discharges of electricity between the earth and clouds, analogous to those of a Leyden jar or pane; the air performing the part of an electric in place of the glass, while the cloud acts as a coating.

It follows that the phenomena above mentioned as liable to arise between oppositely electrified bodies, may be expected to take place between the clouds and the earth, with effects as much exceeding those produced by human agency, as the snap and spark of an electric battery are exceeded by thunder and lightning. If in the one case pith balls and other light bodies are lifted, in the other, water, trees, houses, haystacks and barns may be powerfully affected.* If from a point electrified by a human contrivance, a blast of air is induced; it is assuredly not unreasonable to ascribe to the analogous electrical apparatus of nature, aided by the elasticity of the air, a vertical hurricane. It was under the well founded impression that lightning may be superseded by a current, that we have been instructed by Franklin, to surmount our lightning rods by metallic points, by which electrical discharges from thunder clouds are expected to be conveyed to the earth gradually, which might otherwise pass in sparks of lightning of a formidable magnitude.

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* Fig. 55, Plate VII, affords an illustration in miniature of the rise and fall of bodies situated between oppositely electrified sur-

If, then, it be demonstrated that a continuous discharge of electricity may become the substitute for lightning, and that within the sphere of the discharge the air may be so lifted as to counteract its gravity; it is in the next place only necessary to advert to facts perfectly well known, in order to point out a cause of acceleration sufficient to account for the well known violence of the tornado.

At the height of fifteen miles, the air has been ascertained to have less than one-thirtieth of the density of the stratum next the earth. Of course this substratum would exercise a force nearly equal to the atmospheric pressure, or about fourteen and a half pounds to the square inch, in order to attain the space occupied by the rare medium, to which allusion has been made. It follows that if the weight of the superincumbent air were removed or counteracted, that the inferior stratum would rise with explosive violence.

While the air is thus carried upwards by the concurrent influence of electrical attraction, and the reaction of its own previously constrained elasticity; other bodies are lifted, both by electrical attraction, and the blast of air to which it gives rise. Hence houses within the sphere of the excitement are burst by the expansion of the air which they contain, their

faces, which in the gigantic operations of nature, are conceived to be the exciting cause of the tornado. The phenomena represented by it are designated in Pixii's catalogue as "grelé electrique," and may be thus explained. A metallic rod supports one ball within the bell glass, another without, so as to be in contact with the knob of another rod R, proceeding from the conductor of an electrical machine in operation. The brass ball being by these means intensely electrified, attracts some of the pith balls which lie upon the metallic dish in which the bell is situated, and which should communicate with the cushions of the machine. As soon as the pith balls come into contact with the electrified ball, becoming similarly excited, agreeably to the general law they recede from each other and are attracted by the oppositely electrified dish. Reaching the dish, they attain the same electrical state as at first, and are, of course, liable to be attracted again.

walls being thrown outwards, and their roofs carried away; while, by the afflux of the atmosphere requisite to the restoration of its equilibrium, trees, houses and other bodies are thrown inwards towards the vertical current, from before, as well as from either side.

When once a vertical current is established, and a vortex produced, I conceive that it may continue after the exciting cause may have ceased to act. The effect of a vortex in protecting the space about which it is formed, from the pressure of the fluid in which it has been induced, must be familiar to every observer. In fact, Franklin ascribed the water spout to a whirlwind produced by the concourse of the atmosphere to a given point. His hypothesis was, as I conceive, unsatisfactory, because it did not assign any adequate cause for the concentration of the wind, or for the hiatus which was presumed to be the cause. This deficiency is supplied, if my suggestions be correct.

One fact, of which I am myself a witness, cannot be explained without supposing a gyratory force. About six feet of a brick chimney, without being thrown down, were so twisted on the remaining inferior portion as to be left with its corners projecting.

I have hardly deemed it necessary to advert to the cause of the progressive motion of a tornado, since that would appear evidently due to the current of the atmosphere within which it may be created.

I believe that the electrical excitement which gives rise to atmospheric discharges of electricity, in whatever form they may occur, is usually ascribed to the chemical changes taking place in the atmosphere; especially the formation or condensation of vapour.

Another view of this subject has suggested itself to my mind. It is known that the atmosphere acts generally as an electric, while the earth acts as a conductor of electricity; and since the electric fluid passes through an exhausted receiver with great facility, it results that the rare medium which exists at a great elevation, is equivalent to another conductor. Hence it is evident that there are three enormous concentric spaces, of which that which is intermediate contains an electric, to which the others may act as coatings. When the tendency of electric fluid to preserve an equilibrium is taken into view, I believe myself justified in the inference, that not only the space occupied by the globe, but the region beyond our atmosphere, or where the air is sufficiently rare to act as a conductor, must abound with electricity. Thus the atmosphere is situated between two oceans of electricity, of which the tension may often be different. Between these electric oceans, the clouds, floating in the non-conducting air, must act as movable insulated conductors; and from the excitement consequent upon induction, chemical changes, or their proximity to the celestial electric ocean, must be liable to be electrified differently from each other, and from the terrestrial electric ocean.

The phenomena of thunder storms may arise, from the passage of electricity from one electric ocean to the other being facilitated by an intervening accumulation of the clouds, or in consequence of discharges from one insulated congeries of clouds to another through the earth.

The aurora borealis may arise from discharges from one ocean to the other of electricity, which, not being concentrated by its attraction for intervening clouds within air sufficiently dense to act as an electric, assumes the diffuse form which characterizes that phenomenon.

Falling stars may consist of electric matter, in transitu between one portion of the celestial electric ocean and another, tending to restore the equilibrium when disturbed. They may, in fact, consist of electric matter, passing from one mass of moisture to another; as it may be imagined that in an expanse so vast, in which the tension is so low, there may be a great diversity as respects the quantity of moisture existing in different parts. Indeed, it may be conceived that at times the clouds, insulated from each other, may make their reciprocal discharges through the region occupied by the celestial ocean.

I have been informed by my intelligent friend, Mr. Quinby, who resided for some time in Peru, at an elevation of fifteen thousand feet above the level of the ocean, that the clouds in that elevated region are far more electric than in the lower country of the same latitude; and that, on this account, it was considered as dangerous, at times, to travel in the "*sioras*," or table land. Possibly thunder storms are more frequent in warm weather, in consequence of the greater elevation which the clouds then attain, and their consequent approximation to the celestial ocean of electricity.

Consistently with the hypothesis which I suggested in my essay on the gales of the United States, the enduring rains which accompany those gales are attributed to the contact of an upper warm and moist current of air, with a lower current of the same fluid at an inferior temperature, and moving in an opposite direction. It would follow that, on such occasions, the electricity of the upper region would be diffused among the clouds within the upper stratum, without reaching those existing within the lower current. But in such cases neither stratum would be sufficiently insulated and restricted in its extent to transmit the electricity in a concentrated form, or to be liable to the intense excitement necessary to produce a tornado or lightning.

Facts and observations respecting the Tornado which occurred at New Brunswick, New Jersey, in June last, abstracted from a written statement made by JAMES P. EPSY, M. A. P. S.

BY THE AUTHOR OF THE PRECEDING ARTICLE.

The tornado was formed about seven and a half miles west of New Brunswick, and, moving at the rate of about twenty-five or thirty miles in an hour, terminated suddenly at Amboy, about seventeen and a half miles from the place of its commencement. It appeared like an inverted cone, of which the base was in the clouds, and the vertex upon the earth. It prostrated or carried off every movable body within its path; which was from two hundred to four hundred yards wide. Trees which were embraced successively within its axis were thrown down in a direction parallel to its path; those on either side always pointing towards some point which had been under its axis. Houses were unroofed, and, in some instances, unfloored; in others, their walls were thrown down outwards, as if burst by an explosion. There are two facts stated by Mr. Epsy, and confirmed by Professor Bache, which demonstrate fully the existence of an hiatus. In a house which was exposed to the vertical influence of the tornado, a sheet was lifted from a bed, and carried into a fissure made in the southern wall, which subsequently closed and retained it. The same result was observed in the case of a handkerchief, similarly fastened into a fissure in the northern wall. In some instances, frame buildings were lifted entire from their foundations. Joists and rafters were torn from a house and thrown down at the distance from it of about four hundred yards, and in a direction opposite to that in which the trees not lifted from the earth's surface were prostrated. Of course lighter bodies, such as shingles, hats, books, and papers, and branches and leaves of trees, were carried to much greater distances. There was no general rain, but hail and rain accompanied the fall of the other bodies. The tornado lasted, in any one place, for but a few seconds: the whole of the damage done at a farm having been accomplished, as the farmer stated, while he was passing from the front to the rear of his mansion, so that, by the time that he reached the back door, there was a perfect calm. Meanwhile, his house and barn were unroofed, and all the neighbouring trees thrown down. The noise which accompanied the phenomenon was by every witness described as terrific, being best exemplified by the rumbling of an immense number of heavy carriages. Every object in its path was bespattered with mud on the side towards that from which it advanced. Houses looked as if roughcast, and individuals were so covered with dirt as to be disguised.

Some thunder and lightning attended the tornado. Some trees, which resisted the onset, yielded subsequently; and hence were piled upon those which had fallen earlier. The weaker trees were undermost, and pointed in the direction in which the tornado approached; while the stronger were on the top, pointing in the direction in which it moved away.

Four different places were noticed, where all the trees lay, with their summits directed to a common centre. In the middle of one of these localities, the house was unroofed, and the handkerchief and sheet were lodged within the fissures in the walls, as already stated. The windows in the same house were all broken, and much of the glass thrown outside. From the evidence, Mr. Epsy infers that the apparent height of the tornado was about a mile. He states that there were, on the same day, two other tornadoes about seventeen miles apart; and of which the nearest was about the same distance from that of New Brunswick. He conceives that the phenomena all concurred to demonstrate an "inward motion from all directions towards the centre of the tornado, and an upward motion in the middle." These statements of Mr. Epsy are confirmed by Professor Bache.

One fact of some importance has not been mentioned by Mr. Epsy, which was observed by persons who were upon the ground during, or soon after the catastrophe. I allude to the partial withering of the foliage of those small trees or shrubs, which, from their suppleness, were like the reed in the fable, neither uprooted nor overthrown. This unpleasant effect was perceptible when I visited the scene. Each leaf was only partially withered. As it would be inconceivable that mechanical laceration could have thus extended itself equably among the foliage, a surmise may be warranted that the change was effected by the electricity associated with the tornado.

Concluding Remarks by the Author of the Article.

I ought, perhaps, sooner to have acknowledged that I am aware that it has often been suggested that water spouts might be caused by electricity; but the conjecture has not, as far as my information goes, been heretofore supported by any satisfactory explanation as to the mode in which such a tremendous power could arise from that source. That I am warranted in this impression, will, I trust, appear evident from the circumstance that two of the most distinguished among the late writers in the department of science to which the subject belongs, seem to admit, or to demonstrate, their inability to afford any explanation. I allude to Pouillet and Despretz.

In his treatise on meteorology, Pouillet introduces two narratives respecting tornadoes, which were analogous in every essential point to that of New Brunswick. Especially the existence of an hiatus is proved by the allegation that the walls of prostrated houses were thrown down outwards. A labourer was first urged forward, in the next place lifted, and lastly overthrown.

The learned and ingenious author concludes with these remarks.

"Comment cette puissance, quelquefois si prodigieuse, peut-elle prendre naissance au milieu des airs? C'est une question, il faut de dire, à laquelle la science ne peut faire aucune réponse précise. De toutes les conjectures vagues et hasardées, que l'on peut faire sur l'origine de ce météore, la moins invraisemblable est peut-être celle que le regarde comme un tourbillon d'une excessive intensité. Mais une discussion sur ce point nous semblerait prématurée; il faut multiplier les observations, et constater avec plus de précision toutes les circonstances de ces phénomènes."—*Elemens de Physique Experimentale et de Météorologie, vol.* II, *p.* 727.

All the information respecting tornadoes afforded by Despretz is comprised in the following paragraphs, which I quote in his own words.

"*Trombe.* La trombe se montre en mer et sur la terre; tantôt elle semble sortir du sein de la mer, et s'élève jusqu'aux nuages; tantôt elle descend des nuages jusqu'à terre.

"C'est une colonne d'eau cônique qui tourne sur elle-même avec une grande vitesse; elle a quelquefois jusqu'à plus de deux cents mètres de base. Elle est très-commune entre les tropiques: les navigateurs passent rarement près des côtés de Guinée sans en apercevoir plusieurs.

"Les trombes produisent des effets terribles; elles déracinent les arbres, renversent les foibles habitations, soulèvent les voitures, etc.

"On peut se faire une idée des trombes par les tourbillons de pouissèire qui se forment tout à-coup, en été, sur les routes, et qui tournent sur eux-mêmes avec une grande rapidité."—*Traité Elementaire de Physique, paragraph* 656, *page* 828, *par. C. Despretz.*

In Nicholson's Journal, quarto series, London, 1797, vol. 1, page 583, there is an interesting account of some tornadoes seen from Nice, illustrated by engravings, by M. Michaud, who appears to consider them as the effect of electricity, and infers that he could produce the phenomenon in miniature by the aid of a machine, as thunder and lightning are by the same means illustrated. This I have found to be erroneous, as far as my experience goes, and from a cause which is, agreeably to my hypothesis, quite evident. I mean the absence of the co-operating influence of the air when emancipated by electric action from the confinement arising from its own weight.

The theoretic remarks of Michaud are very brief, and, to me, scarcely intelligible, as he does not inform us in what way he supposes the electric fluid to operate.

I have understood, since I conceived my hypothesis, that Becoaria ascribed water spouts to electricity, but I have not had the advantage of learning by what reasoning he justified his inferences. However, should it appear that I have made, through the want of information, any undue claim to priority, I shall cheerfully do justice to any philosopher whose speculations I may have overlooked.

XXXV. *On Electro-magnetic Coil Machines. By* Mr. J. C. Nesbit. *In a letter to the Editor.*

Sidney Street, Manchester, Feb. 10, 1838.

Dear Sir,

It is with sincere pleasure that I take up my pen to address a few lines to you, and must apologize for not doing so before, but I have been so very much engaged since you were in Manchester, that I have not had time.

Your lectures in our Institution have induced many of our members to engage in the study of Electro-magnetism; and they have stirred up a spirit of enquiry and a desire for philosophical knowledge, which cannot fail to be attended with beneficial effects. When you had been gone about a week, it struck me that the reason why your machine for giving shocks from the secondary coil, failed in fastening any person to the handles when the velocity of the wheel was great, was on account of an imperfection in the method of making and breaking the connexion, and not for the reason you alleged; namely, that when the connexion was broken so quickly that the polar magnetic lines emanating from the iron within the thick coil met the returning ones, and so neutralized each other, and consequently produced no action on the outside coil of wire.

I conceived if it had been the case in that instance, it ought to have been the same with a magnetic electrical machine; but the breaks in that instrument may be made several thousand times in a minute. I accordingly made a machine with a different break and which answers remarkably well.

A and B, fig. 54, plate VII, are two brass pillars with nut screws. The wires from the battery are connected with the pillars by putting the ends of them into the small holes in the top of the pillars, and they are closely pressed by means of the nut screws, so as to form a perfect metallic contact. One end of the thick coil is connected to the underpart of the pillar B; the other end of the coil is joined to the pillar C; from thence a stout bent wire proceeds to the break at K; the wire W proceeds from the metal axle, on which the break is fixed, downwards, through the stand, and is connected underneath with the pillar A, which completes the circuit. The pillars D and E are connected with the two ends of the thin wire. The break which I first employed was a rachet wheel the same as Y.

The inner coil consisted of 400 feet of thick wire, and the outer coil of 1700 feet of thin wire, and the diameter of the hollow axis of the bobbin was one inch. With a battery of about half a square foot of surface, and with a bundle of *very thin* iron wire in the axis of the bobbin, the shock was most intolerable. Six persons took hold of hands (having moistened them with salt and water), and were connected with the handles X and Z. The battery was in vigorous action. On working the machine, its power was such that the persons hold of hands found it impossible to leave loose, notwithstanding their utmost endeavours to do so. A very bright spark is observed at the break, and it is a deal larger than when there is no wire in the axis of the bobbin. The only fault of this first break was, that it made a great noise from the spring and catch of the thick wire on its teeth. To remedy this, a piece of brass, about twice the thickness of a penny-piece and about two inches in diameter, had eight holes a quarter of an inch in diameter each, drilled in it at equal distances near the circumference: these were plugged up with hard wood, it was then put into a lathe and turned until the wood and brass were nearly equal. It was also slightly grooved round the circumference to receive the wire from the pillar C.

This break answered all my expectations; there was no noise, and the shocks were as strong as ever. A little oil put on the break diminishes the friction and still allows the current to pass. The groove of the break must occasionally be rubbed with sand paper. The use of mercury is also completely superseded by the brass pillars, which answer as well or better.

On the whole I think that the above is one of the best forms the machine is capable of taking. The break can be applied to any other arrangement as well as to this.

I remain, my dear Sir,
Yours, very truly,
To W. Sturgeon, Esq. J. C. NESBIT.

Answer to Mr. Nesbit's letter.

My Dear Sir,

It gives me much pleasure to hear that my Lectures have been so useful to the members of your Institution. The beauties of Electro-Magnetism, and Magnetic Electricity, only want to be made known by demonstrative experimental illustration, to become generally and duly appreciated. There are no phenomena more interesting, nor more deserving the attention of the cultivators of science than those embraced in these two kindred branches. But it is to be feared that much of their beauty and simplicity have been obscured by the unnecessary habiliments of mystery with which they have been clothed, even where conspicuity and clearness of illustration might have been expected.

With respect to the comparison which you make of the Magnetic electrical and the Electro-magnetic coil machines, I think you have been led into some error. The mode of exciting the enclosed iron in the one, is very different to that of the iron in the other. In the former instrument the excitation is by means of an already polarized magnet; but in the latter the exciting magnet, (the *true electro-magnet* of the inner coil) has itself to be formed, before it can possibly operate on the enclosed iron. Hence you will perceive that, with respect to the Electro-magnetic coil machine, the magnetic *exciter* of the iron has to be *called into existence* every time the primitive circuit is completed. This process requires time. Moreover, when it is considered that the formation of an electric current also requires time, it is probable that when the opening and closing of the battery circuit is performed with great rapidity, the electric fluid never gets into a full current; or if it does, it has not sufficient time to operate on the magnetism of the iron to arrange it to the full extent.

The full excitement of iron, even by a permanent magnet, also requires *time*; and although the strength of the shocks taken from a magnetic electrical machine, *appears* to bear some proportion to the rapidity with which the machine moves, that would be no reason for supposing the magnetic excitement to bear the same proportion. When the iron revolves rapidly, the magnetic excitement of it is nothing like so great as when it moves slower. The *apparent* formidableness of the shocks is no criterion of the *real power* of the machine. Much of the sensation depends upon the rapidity with which the discharges succeed one another in assailing the person operated on. Chemical decompositions, deflections of a magnetic needle, and Electro-magnetic rotations, are exhibited to the greatest advantage when the rotation of the iron is at a moderate speed. A very great velocity invariably lessens all these operations.

Let me now again call your attention to the Electro-magnetic coil with its enclosed iron. You tell me that your mode of opening and shutting the circuit answers very well. Of the truth of this there can be no doubt; but you have not said that, by means of it your machine operates best with the greatest velocities of the wheel. Have the goodness to make a series of experiments on this point, observing to have a solid cylinder of iron in the axis of your coil; and let me know the results, in your next letter. I shall at all times take a pleasure in forwarding your scientific enquiries, and in placing the result of your labours before the readers of this Journal.

Your ingenious method of covering wire for these purposes, is highly valuable to all those engaged in this branch of science, and the description of the process which you are about to favour me with, shall be immediately placed before the readers of the "Annals." You have not said that the wire of the coil which you have made, is covered in that manner; but if not, it will be gratifying for you to know that the specimen of wire which you gave to me, whilst at Manchester, answers very well. Iron wire, covered with cotton, such as I purchased, will not answer for making these instruments, unless covered with varnish. The metal rusts, and discolors the covering, and would eventually destroy it. For permanent instruments, no wire seems so well adapted as that of copper.

The motive for this public reply to your letter is, that the hints which it contains may possibly be of service to others who are engaged in similar pursuits; and to encourage them in bringing forward the fruits of their scientific labours.

I am, dear Sir,
Your's, very truly,
W. STURGEON.

To Mr. J. C. Nesbit.

XXXVI. *On the Electro-magnetic Machine.* *By* G. H. BACHHOFFNER, ESQ., *Lecturer on Chemistry to the Artists' Society, &c.*

The important facts likely to result from the addition to our stock of apparatus by the introduction of the Electro-magnetic machine, must, I think, be evident to every experimentalist, either chemist or electrician. The age of the oldest (now numerous) modifications,* is I believe little more than a twelvemonth, and when we review the interesting phenomena already made known, it fully warrants our anticipating far more important results than have yet been obtained.

In the electro-magnetic machine which passes under my name, I lay claim to no merit of invention beyond that of adapting to my own particular wants, a form, which I find to answer my purpose. If, in an arrangement of this kind, we chance to devise one, the construction of which has not been made before, then I can see but little impropriety in permitting the name of the designer to accompany it. Exactly in this situation stands the machine† in question; which, so far as I am concerned, will, I trust, suffice for the several disputants for the priority of invention.

The theory of its action at the present time interests me but little; my aim being rather to ascertain its powers than to enquire into the cause of effects, when those very effects are at present in so crude a state. I am therefore content to take either the theory of induced currents first promulgated by Dr. Faraday, or that of Mr. Sturgeon, in which the phenomena are explained by the expansion and collapsion of the tangential lines; as it appears to me they both amount to much about the same difference, as that existing between the two theories of electricity, wherein the experiments in support of the one are readily taken up by the other, with a view to the settlement of the question: and, therefore, until we know more of the nature of the fluids (if fluids they be) we are thus so freely speculating upon, I am quite content to rest in the situation of a silent spectator until the point shall have been incontestibly decided; at the same time I cannot but admit that. I am strongly impressed with the idea of the individuality of that property hitherto called "Magnetism," which, according to the celebrated Ampère, is entirely due to the action of electrical currents.

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* The Rev. Professor Callan's Repeater may fairly be considered as the first, at least it was from an inspection of that instrument I was led to the construction of my machine.

† This machine was exhibited to the members of the Electrical Society both before and after the chair was taken, at the meeting held on August 5, 1837, and was, I believe, the first of the kind exhibited in public, the description of which was read to the Society at its meeting of the 12th of the same month.

Since the introduction of this machine, I have laboured diligently with it, from the conviction that it will eventually become a most important instrument in analysis, to the almost entire exclusion of the voltaic battery, beyond the mere exciting influence of the primary current; but, hitherto, I am constrained to add, that I have not succeeded to my expectation. To enter into the detail of the several series of experiments which I have been engaged upon, would occupy too much valuable space of the Annals; and at the same time tire its readers: I shall therefore give merely the results, which may serve as a nucleus for other experiments.

The electromoter employed demands our first attention. This should consist, most unquestionably (if the instrument is to prove of any value*), of a single pair of plates. My early experiments were all conducted with one of the small pot batteries described in Vol. I., page 213. Although this answers very well, yet a pair of plates arranged as in the calorimotor of Professor Hare, and charged with dilute acid, answers much better. If, however, a continuous and steady action is required, then the former arrangement is decidedly preferable. The amount of exciting surface will, however, much depend upon the extent and thickness of the conductor, through which the current is intended to travel. That a certain ratio will eventually be determined upon admits but of little doubt, this however has not, as far as I am aware of, yet been enquired into; I have made several experiments for this purpose, but they are not sufficiently accurate to enable me to offer them to notice. The following experiment first called my attention to the subject. My large coil contains about 400 or 500 yards of covered wire forming the channel for the battery current. When the small pot sustaining battery is connected with the former, in the circuit of which a rotating magnet is placed, the latter will be found to rotate but very slowly, the length of the conductor appearing to offer some resistance to the direct passage of the current, if such can be admitted after the interesting experiments of Professor Wheatstone, on the velocity of the electric fluid through good conductors; if not, I am at a loss for an explanation of the phenomenon. Remove the long coil and substitute one of about 150 yards, the rotations are instantly increased, accompanied with a more brilliant evolution of light as the points leave the mercury.

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* The grand object being that of effecting by the aid of this instrument, those electrolytic effects at present obtained from an extensive voltaic surface, when arranged so as to afford what is commonly called *Intensity.*

Certainly the most interesting action displayed by these machines, is that of their electro-chemical or electrolytical effects, although their energy at the present time is extremely feeble when compared with any compound voltaic arrangement however low its intensity. In a paper read by Mr. Golding Bird at the Electrical Society, he stated that he obtained very powerful electrolytic effects from the secondary or induced current, and that these effects were considerably augmented when the bundle of *insulated* iron wires, suggested by me, were employed in lieu of the bar of iron. That paper, I see by reference to the January number of the Phil. Mag., is published in that Journal, in which he states, that rapid electrolytic action ensues whenever the terminations of the long thin coil are arranged for that purpose. That effects are produced by the action of the induced current, I am well aware of, but in all my experiments, instead of being as Mr. Bird describes them (viz. rapid), were so feeble that it required, as in the electrolization of water, almost the aid of a lens (let the primitive current be ever so powerful), to ascertain their existence. This statement is not drawn from mere off-hand experiment, but is the result of many, several of which have been carried on for hours, and in some cases, days together. These results are certainly in opposition to the generally supposed action of so powerful a current of electricity, as evinced by its physiological effects,* if the one bears any relation to the other.

The electrolytic effects which I have obtained, have always been from the battery or primary current, bearing in mind also that such have been produced by the exciting influence of a single pair of plates; of course the employment of a compound battery would in this case defeat the object. In fact the electrolytic energy of every compound series that I have tried, is greatly reduced by the introduction of a coil of any extent of wire, such as 200 or 300 feet into the circuit. This fact, was, I think, distinctly proved in a paper which I had the honour to read before the members of the Electrical Society, wherein the fact was tested by experiment. The action of the coil is in this respect very singular, for while on the one hand it reduces the energy of a compound circle, it on the other elevates a single pair of plates however small,* to the position occupied by the former.

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* Does the natural quantity of electricity peculiar to the animal economy, contribute in any way to the augmentation of these effects, for the whole quantity I should presume is in motion, when the body forms part of the circuit?

The mode which I employ for obtaining these effects, is as follows:—Two wires branch off from that part of the battery circuit where contact is intended to be broken; then by suddenly throwing the whole of the current circulating in the battery and coil, through the electrolyte, which is readily done by breaking contact, the decomposition is effected. In all my experiments the effects produced by this mode of manipulation, are by no means inconsiderable. Thus when one of the small pot batteries, before mentioned, is employed, it appears as near as I can estimate, to be very little, if any, inferior to 10 pairs of Wollaston's plates charged with salt and water.

The calorific effects seem to depend upon the energy of the electromotor, and are not, I consider, increased by the introduction of the coil; on the contrary, should the calorimotor employed be small or only weakly charged, I have observed a considerable decrease when the coil has been introduced. The combustion of the metals when reduced to the state of leaves, receives on the contrary a considerable increase. This perhaps may be rather apparent than real, the brilliancy of the experiment being greatly increased, when the coil forms part of the circuit, depending upon the copious flood of light evolved on breaking contact. The combustion and oxidation of mercury is also considerably increased by the same means. Charcoal, on the contrary, is not much effected: thus if the battery is not capable of igniting the charcoal prior to introducing the coil, it will not do so when the latter forms part of the circuit. The introduction of the coil into the circuit of a powerful calorimotor, however, greatly alters the character of the light evolved by the charcoal points.

The brilliancy and increase of the spark at breaking contact, is a fact of great interest, particularly when a powerful calorimotor is employed, and the spark taken from different metals, the evolved light affording the peculiar colour which each present when undergoing combustion.†

Silver gives out a greenish light, copper a blueish green, lead a violet, and zinc, which is by far the most brilliant (probably from the combustibility of the metal), a blueish white light; the latter may I think eventually be applied for the purpose of illumination, instead of gas or oil, both on the score of brilliancy and economy; the scintillations thrown off from a steel file are also extremely brilliant and interesting; steel being far more so than iron.

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* Thus I have obtained very decided effects, when only the celebrated thimble battery of Dr. Wollaston was employed as the electromoter.

† In these experiments, it is necessary to employ friction, in order that slight portions of the metal may be detached at each break. These appear to undergo combustion.

The spark when taken from a solid body, as a wire for instance, assumes, when an extensive exciting surface is employed, a very curious appearance; for, in addition to the brilliant light, the latter is surrounded by a dusky red flame, differing very materially from the absolute spark; this is best shown by holding the two wires, where contact is intended to be broken, in the hands, then by briskly drawing one wire over the other, pressing it firmly as it leaves the under one, which should terminate in a point. As far as I am enabled to judge, it is this red flame* only which ignites inflammables, with the exception of hydrogen, the latter being instantly ignited by the most minute spark. To illustrate the above by experiment, pour a few drops of ether on the surface of some mercury, and take the spark from the surface of the latter, through the ether, the mercury will become oxidized, and at the same time appears to undergo combustion, but the ether, if the experiment is carefully performed, will not be inflamed; but if the ether be poured on a metal plate, or in the bowl of a spoon, and the spark be taken by briskly drawing a wire over it so as to produce the red flame, the ether is instantly ignited.

I have not been so fortunate as Mr. Golding Bird in obtaining that decided electrolytic action of the secondary or induced current, which he states has been obtained by him. This may perhaps be owing to my unskilful manipulation; but I believe I am not singular in this misfortune, for I am not aware that the same effects have been obtained by any other experimentalist.† I think, if I remember correctly, Dr. Faraday was equally as unsuccessful in his experiments; at the same time I do not despair of the instrument becoming as I before stated of great importance in analytical research. It is now far superior to any magneto-machine which I have had an opportunity of examining, and I should think will ever remain so, for the effects of the latter apparatus, much, I should consider, depends upon the exciting influence of the magnetism of the steel, while these machines cannot be said to labour under any such restrictions.

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* I use the term flame by its similarity to the flame of any combustible, and from its difference in shape, to that of the spark; the former appearing as a brilliant star, the latter as a brush or pencil of light.

† The Editor may perhaps remember having seen at my house during last summer, the feeble action of the secondary current. I have not been able since to increase that action.

Mr. Golding Bird, in his paper before alluded to, appears, I consider, to raise a very futile objection to these machines, simply because they require turning; we may as well object to the electrical machine, or the air pump, by the same rule. To obviate this inconvenience, that gentleman has constructed an instrument to which he has given the name of "magnetic contact breaker," made by Mr. Neeve, of Great St. Andrew's Street, Seven Dials; from whom I also obtained the above instrument. Now, one of the principal points in favour of my machine, and which has long been a disideratum in electro-magnetic apparatus, is the total absence of mercury connexion, not one atom of that metal being required. In the apparatus of Mr. Bird, we have four shallow mercury cups, the mercury in which is constantly undergoing combustion, attended with the volatilization and the formation of thick coats of oxide on the surface. This inconvenience is at once estimated by every experimentalist. Again, his apparatus requires a very nice adjustment of the poles of the magnets, which, if at all displaced, immediately stop the action. The instrument is certainly an interesting addition to our stock of electro-magnetic apparatus: but, in my opinion, falls very far short, in point of convenience and utility, of the rotating electro magnet, which, I believe, after the spur wheel of professor Barlow, was the next self-acting or magnetic contact breaker employed. This office it very effectually performs without one tenth part of the trouble of Mr. Bird's apparatus.

I have, however, a far more important objection to urge against all these self-acting contact breakers: viz.—that they cease to act the moment we attempt to estimate the electrolytic action of the primary current; and, therefore, the most powerful effects are entirely useless if they are employed: for, as I before stated, the effects developed by this current, be the electromoter ever so small, surpass, in an eminent degree, that of the secondary.

In the arrangement of the coils, I have followed the plan originally suggested by the rev. N. J. Callan, in the construction of his large electro-magnet. This form was much improved by Mr. Sturgeon's plan of coiling the same quantity of wire in one compact coil, on a reel or bobbin, which is the mode I have employed: but in no case do I permit the primary and secondary to be in metallic contact with each other, care being taken to keep them distinct by appropriate insulation.

I have now several of these compound coils, from the size of a common cotton reel up to my large one, which contains nearly 2,000 feet of wire, the smallest of which, when excited by a battery of about four square inches, communicates a shock scarcely supportable, and the primary current will also afford electrolytic action.

In all my experiments I have invariably employed the bundle of *insulated* iron wires. Mr. Bird mentions soft iron wires; but I presume he means covered or insulated wire, at least such he employed in his experiments at the meeting of the Electrical Society.* If the wires be not insulated, very little if any increase of power is obtained.

I would observe that, if the wires are surrounded by a scroll of tinned iron, it is evident that in that case their effects are entirely destroyed, for the simple and obvious reason, that they convert the tube of tinned iron into a solid bar, or what is equivalent to a solid bar so far as their action on the coil is concerned: in which state I should consider, in accordance with the experiments of Messrs. Barlow and Sturgeon, an absolute decrease in power would be the result. The extraordinary increase of power displayed when this insulating bundle is employed by itself, is a fact not easily understood; for I find the aggregate of the several temporary magnets, when arranged for sustaining weights, falls very far short of the solid bar commonly employed.

The best form of the bundle is separate pieces from eight to ten inches long, bound firmly together at each end. At the present time, these machines are in the hands of but few experimentalists: when, however, they become better known, there is every reason to hope for very important results.

I have tried several different arrangements of the coil and electromoter, but do not find that any of them are superior to the mode before described. The last was that of introducing a separate coil between each pair of plates of a compound battery of six pairs of elements, of four square inches: the result gave no better effects than the same quantity of wire would do, as when arranged in one compact coil.

13, *Aberdeen Place, Maida Hill,*
Feb. 14, 1838.

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* The fact of my having first introduced the insulated bundle of wires either escaped the notice of Mr. Bird, or was considered by him a circumstance of too trivial a character to be mentioned, notwithstanding the great increase of power obtained.

XXXVII. *Experiments in Electro-magnetism.* By CHARLES G. PAGE, M.D.*

To Professor Silliman.

Dear Sir,

I noticed in the July number of the Franklin Institute Journal, an announcement of the discovery of the thermo-electric spark by an Italian philosopher, and also the subsequent exhibition of the spark by Professor Wheatstone to Faraday and others; the date of the discovery is not given. On referring to my notes I find that I obtained a spark in August last, but not the shock. The spark and shock were both obtained December 2d., 1836, and exhibited to a number of friends, and announced in your last number. It appears that the European philosophers have not yet obtained a current of sufficient magnitude to afford a shock by the multiplier, although they use in the experiment a great number of pairs. In my experiment only a single pair is used either of bismuth and iron, bismuth and zinc, or bismuth and antimony, and yet the induced or lateral shock given by the multiplier is very distinct by acupuncture. The particular arrangement of the thermo-electric elements to produce such powerful effects, I do not wish to describe at present, as I hope ere long to announce it as a substitute for galvanic batteries in many experiments.

On the disturbance of Molecular forces by Magnetism.

A short article on this subject appeared in the last number of this Journal, under the caption *Galvanic Music.* The following experiment, (as witnessed by yourself and others a long time since,) affords a striking illustration of the curious fact, that a ringing sound accompanies the disturbance of the magnetic forces of a steel bar, provided that the bar is so poised or suspended as to exhibit acoustic vibrations. An electro-magnetic bar four and a half inches in length, making five or six thousand revolutions per minute near the poles of two horse shoe magnets properly suspended, produces such a rapid succession of disturbances, that the sound becomes continuous, and much more audible than in the former experiment, where only a single vibration was produced at a time.

On the application of Electro-magnetism as a Moving Power.

Late in the fall of last year, (November) I commenced the investigation of this subject, not knowing that anything more

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* From Silliman's Journal for October 1837.

had ever been effected than what appeared in an instrument before me at that time, viz. Ritchie's revolving Galvanic Magnet, which consists of a horizon talbar of soft iron covered with copper wire, the ends of the wire descending into mercury cells.* This instrument was the basis of my pursuit. Finding that this bar never attained its maximum velocity, from the occasional union of the battery poles, I soon remedied this defect by a contrivance, wherein the bar moved vertically, and the mercury cells were entirely independent of each other. The instrument thus improved became an interesting and useful piece of apparatus, and is in fact the revolving interrupter described and figured in the last number of the Journal. The stationary magnets, instead of being single contrary poles, at opposite sides of the circle described by the bar, were multiplied so as to form an entire circle of poles, with the exception of an inch on each side between the opposite poles. The magnets were short bars arranged in the form of a cylinder, somewhat like the staves of a barrel, and the poles not in use were united by armatures of soft iron. The velocity of this model was very great, but I found the scattering and oxidation of the mercury a great inconvenience and soon substituted for it solid conductors. The wires on the bar had their similar ends united by single wires, which were brought down and soldered by cylindrical segments of metal, firmly fixed upon, but insulated from the axis. These segments representing the ends of the wires covering the revolving bars, were insulated from each other by pieces of horn or ivory. Two wires connected with the poles of the battery pressing against these segments with a spring, furnished sufficient metallic contact to ensure the passage of the galvanic current through the wires from end to end. As the segments revolved, they presented opposite ends of the wires to the fixed battery wires and thus the poles were changed.† But the most important discovery in relation to the application of this power, is the

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* This apparatus is described in Vol. I. p. 112, fig. 32, Plate V. of the Annals of Electricity, &c.

† Before the appearance of the April number of this Journal (Silliman's,) in which Davenport's machine was partly described, I addressed a letter to Professor Silliman, to learn if he was aware of any experiments of the kind hitherto made. His answer was, "the best imformation you can have on this subject, will be embodied in the coming number of this Journal." The journal appeared with a description of Davenport's machine, but the mode of making battery connexion and changing poles was reserved, and until within a short time since, I supposed that mercury was the medium. Finding lately that he used dragging wires upon semizones of metal, I have ecured the above arrangement to myself.

following, viz.: the admissibility of oil between the solid conducting surfaces. After the machine had revolved for a time, I found it necessary to free the revolving segments, (or discs they may be called) from oxide, even when it was made of silver, gold, or platinum. Amalgamating the surfaces, the oxide collected with still greater rapidity. It occurred to me that if the interposition of oil or naptha would not interrupt the current, the oxidation of the rubbing surfaces might be entirely prevented. On trying oil I was agreeably surprised to find that the current was not only not interrupted when the pressure of the metals was very slight, but that it passed with greater certainty, and enhanced the operation of the machine six fold. It appears that oil more than compensates for its non-conducting property, by keeping the surfaces free from oxide.

This discovery will prove of vast importance in the laboratory, as it will dispense with the use of mercury in many experiments, and prevent the constant necessity of amalgamating and cleaning conductors. Having attained such an advantage in small models, I proceeded to the construction of a large one. The revolving bars are a foot in length, and weigh together ten pounds. They are disposed at right angles on the same axis, but revolving in opposite ends of the cylinder of magnets. With steel magnets its power is very great: but with galvanic magnets its power is sufficient to carry a machine for covering copper wire with cotton: and with the addition of more coils of wire, might doubtless be made to turn a large lathe. Now although it is certain that machines of this description may be applied to a considerable extent, yet it is evident that their power is limited. These and all other similar machines must be liable to the same objection, that their magnetic forces cannot be commensurate with their size and weight. This objection I have surmounted, (as far as theory and a small model afford proof) by the following arrangement. Instead of extending large bar magnets through the whole diameter of the circle, I have horse shoe magnets carried near to the circumference of the circle. They are arranged on arms or radii like the spokes of a wheel, and both poles of each horse shoe are in operation at once. They each change their poles four times in each revolution, and the change is effected as before by revolving segments or discs. From the great success of a small model on this plan, I have commenced and now nearly finished an engine on a grand scale, from which I expect great power. The revolving apparatus weighs nearly a hundred pounds. If its power should be in proportion to that of the small model, it must exceed one horse.—*Salem,* August 15th, 1837.

XXXVIII. *Chemical Researches on Dye: by* M. CHEVREUL.

(Extract.)

Introduction to the third, fourth, fifth, and sixth memoirs of these researches.

I propose, in the third, fourth, fifth, and sixth memoirs of my chemical researches on dye, to exhibit the changes that the most general agents, such as pure water, the atmosphere, the light of the sun, and heat, can effect under well defined circumstances, on several colored matters, fixed on stuffs, so as afterwards to discover the influence of the simple forces, capable of concurring in the production of these effects.

If it be generally known with what rapidity certain coloring matters, such as Indian saffron, annotto, seaflower or bastard saffron, archil, &c., change, when the stuffs on which the dyer has placed them receive, in the open air, from the direct light of the sun; nobody to my knowledge, has undertaken to determine the *precise part taken by the light in these phenomena of alterations, by trying if it be capable of producing it when acting alone, to the exclusion of steam, and above all of oxygen,* which are also two causes by which the atmosphere intervenes in many phenomena: nobody, to my knowledge, has undertaken in any other respect than that I have just considered, to determine, by precise observations, if *the same coloring matter fixed on cotton, silk, and wool, is more changeable in one case than in the other.*

It is these researches, followed up in this double respect, for several years, that forms the subject of three memoirs.”

THIRD MEMOIR.

On the action of pure water on stuffs, dyed with different coloring matter.

(Extract.)

Water may be considered in very different views with regard to dying,—my study, in this memoir, will be its relations to the dissolving liquid, with stuffs already dyed, when it acts in separating the coloring matter from them—in modifying or changing them.

Water, at the ordinary temperature, and absolutely deprived of air, placed in contact with dyed stuffs, can only exercise an action on those whose coloring matter is of a nature to be dissolved in it, whether entirely, or as is most common, only in part; thus water would be without action on a stuff dyed with *indigotine*, whilst it would tend to dissolve the sulpho-indigotic acid which would have been applied on another sample of the same stuff, whether alone, or through the medium of the peroxide of tin, alumine, &c.; but in any known case, at ordinary temperatures, pure water will only tend to alter the elementary compositions of the principles that it can dissolve, at least, under the circumstances in which the stuff itself is not changed.

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* From the Comptes Rendus hebdomadaires, des Séances de l'Académie des Sciences. Translated by Mr. J. H. Lang.

I have kept the undermentioned woollen stuffs in distilled water, for a month, without having observed any sensible change:

Wool mordanted with alum, dyed with woad;
Wool ———————— alum and tartar, dyed with woad;
Wool ———————— alum, dyed with yellow wood;
Wool dyed with annotto;
Wool mordanted with alum and tartar, dyed with archil;
Wool ———————— alum and tartar, dyed with Brazil wood;
Wool ———————— alum and tartar, dyed with logwood;
Wool ———————— alum and tartar, dyed with madder;
Wool ———————— alum and tartar, dyed with cochineal;

At the end of three years, the changes might be called insensible, for they were limited to a very light red tint, which the yellows assumed, and a very light brown colour presented by the logwood. I have every reason to believe that this light tint was owing to the action of the atmospheric oxygen which had penetrated the vessel, although stopped up with emery, and filled; and what seems to prove it, is, that the same wool dyes, kept under the same circumstances, in vessels of diluted hydro-sulphuric acid, have not changed; the yellows remaining, and the logwood violet.

I shall add to what I have already said, that after remaining some days in the diluted hydro-sulphuric acid:

The wool, dyed with sulpho-indigotic acid, was completely discolored; it was restored to blue in the air.

The wool dyed with *archil*, was discolored; it was restored to violet in the air.

The wool dyed with Brazil wood, was very much weakened at the end of a month.

The preceding experiments relate to a case in which the weights of the stuffs dyed, were to the water as 1 to 500; but I ought to observe, that things might have been otherwise if the mass of water, in contact with the stuff, for a certain time had been in quantity infinitely great with regard to it.

FOURTH MEMOIR.

On the changes that Indian saffron, annotto, seaflower or bastardsaffron, archil, sulpho-indigotic acid, indigo, and Prussian blue, fixed to silk, cotton, and woollen stuffs, experience from light, atmospheric agents, and hydrogen gas.

(Extract.)

FIRST CHAPTER.—*Experimental Arrangements.*

After having fixed some silk, cotton, and woollen stuffs, in threads or pieces, dyed with Indian saffron, annotto, seaflower, archil, sulpho-indigotic acid, indigo, and Prussian blue, on pieces of pasteboard, we exposed them so as to receive the direct light of the sun, under the seven following circumstances:

1st. In a vessel in which a vacuum had been made, and which also contained chlorure of calcium;

2nd. In a vessel containing air, dried by chlorure of calcium.

3rd. In a vessel of air, saturated with steam;

4th. In the atmosphere;

5th. In a vessel, containing the steam of pure water;

6th. In a vessel containing hydrogen gas, dried by chlorure of calcium;

7th. In a vessel containing hydrogen, saturated with steam

CHAPTER II.—*Results of observations, made to determine the changes that stuffs, subjected to experiment, have undergone with regard to light, atmospheric agents, and hydrogen gas.*

I am about to show, in as many tables of coloring matters as I have examined, the changes that samples of silk, cotton, and wool, dyed with any one of them, have experienced under the seven circumstances mentioned in the first chapter.[*] I shall enumerate at the end of each table, the most remarkable facts that it demonstrates.

To save the reader the trouble of drawing from my observations the consequences deducible therefrom, I shall now examine them under the following heads:

1st. As regards the different coloring matters placed in the experiment, compared with each other in respect to the same stuff, and the same circumstance;

2d. As regards the nature of the cotton, silk, and woollen stuffs on which the same coloring matter is fixed, being under the same circumstances;

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[*] *These tables will be published in the Recueil des mémoires de l'Académie.*

3d. As regards light and ponderable agents, which have caused changes in the same coloring matter, fixed on a similar stuff, but on samples placed under the seven circumstances defined above;

4th. As regards the theory of bleaching.

CHAPTER III.—*Observations explained in the second chapter relative to the different coloring matters compared with each other, having respect to the same kind of stuff, and similar circumstance of treatment.*

Since, at present, a great number of mineral coloring matters are used in dying, and that they are frequently employed, and often in conjunction with coloring matters of organic origin; we must not forget the extreme differences which several of these matters present to one another, differences which are opposed to any reconciliation of them to one single class, which was not the case formerly, when but few coloring matters were used in dying, except those of organic origin; and which distinguished chemists arranged under the same head, considering them either as kindred species, or as simple varieties of a single kind. It is some time since my views were first decidedly against such reconciliations, which confound in one group, as little elevated as is the kind, bodies which differ both by the number of their constituent* elements, and by their immediate composition. In short there are ternaries of them, as in the coloring principle of cochineal, quarternaires as in indigotine; or such as are considered as formed directly from two compound bodies, such is sulpho-indigotic acid. The coloring matters of organic origin, do not differ the least among themselves, with regard to the chemical properties of the most elevated order, for if the most part are neutral to the reactive colors, some, such as sulpho-indigotic acid, possess a sensible acidity; in short, as respects the solvents, we find some that by their great solubility in water, seem to be analogous to the immediate principles which contain a notable quantity of oxygen in proportion to the carbon and hydrogen, while others by their insolubility in water, and solubility in alcohol and ether, appear to approach the greasy or resinous bodies in which the carbon and hydrogen are the ruling elements.

In considering the results of my experiments, with regard to whether the coloring matters, from the manner in which they have shown themselves individually, under the circumstances I have placed them in, ought to be ranged under a similar

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** Considérations générales sur l'Analyse organique et sur ses applications.* Paris, 1824, page 167.

gender, (I do not say species, because there is luckily, at present, no one who would thus put the question), I do not doubt but the diversity of phenomena they have presented, will be found too great to justify a reunion of this order; but whatever this diversity may be, it is the great difference of composition which is essentially opposed to such a reconciliation.

Indigo, put on cotton, silk, or wool, is preserved in vacuo, although under the influence of the light; while Prussian blue, applied to the same stuffs, and under similar circumstances, becomes white.

Indian saffron, applied to the same stuffs, changes in vacuo, under the influence of the light, while archil is similarly preserved.

The variableness of the coloring matters of organic origin, under the circumstances which I have observed it, is very different, relative to the time necessary for it to show itself to the same degree, in the different species of these materials, because conformably to a very common opinion, one may be justified in deriving therefrom a character common to all these species, and fit to distinguish them from the uncolored materials of the same origin. On the other hand, it would be a great error to trust to the stability of the uncolored materials, under the circumstances in which the colored materials change. For among the facts that I can quote, there is a remarkable one, viz., that some fine card board, for tickets, and consequently covered with sized paper, being exposed to the action of the light and the atmosphere, conjointly with colored stuffs, was whitened, at the same time that it acquired the property of *absorbing the ink,* owing to the destruction of the size, which rendered it fit for writing on previous to its exposure to the light and atmosphere.

If we look for the cause which has led to the supposition that the coloring materials of organic origin are more changeable by light (and we must add after my observation, and by the ponderable agents of the atmosphere) than the uncolored ones of the same origin, we shall find that in this circumstance the alteration has been observed on a coloring whose weight was more or less feeble in proportion to that of the stuff it dyed, and that then the coloring material could be altered, as well as a certain quantity of the material of the stuff, without the alteration of the latter becoming sensible like that of the former, the result of which was a *discoloration,* a phenomena striking to every beholder.

This explanation connects several facts, which without it would want correlativeness, if some of them even did not appear to contradict others.

"Thus, indigotine applied to woollen stuffs, so as to dye them deep blues, which are the tones of the indigotine gamut, almost exclusively of the use for our garments of blue wool, passes for one of the most solid coloring principles we know of; for in fact (excepting the whiteness which certain blue cloths present on the seams, or parts of the clothes exposed to friction) the color of the stuff seems to be the same from the time we take it as a *new garment*, until we leave it off as *an old one.* However the appearance is not the reality, for if the indigotine form only a light blue on silk, or for a stronger proof on wool, or even on cotton, this tint is very quickly destroyed under the influence of the light and ponderable agents of the atmosphere; consequently, if we only wore clothes dyed a light blue, with indigotine, we should conclude that this *coloring principle is very changeable.*

If we now consider that in a stuff dyed light blue, there is only a very little indigotine in proportion to the weight of the material of the stuff, we shall, from what has been already said, perceive how a small quantity of indigotine may disappear without the material of the stuff seeming to change in its tenacity and physical properties, its color excepted. If we afterwards consider that in stuff dyed dark blue, there is much more indigotine in proportion to what there was in the light blue, (it is not necessary that there be much in proportion to the weight of the material of the stuff) we shall perceive how it is that a dark blue cloth garment is unserviceable before the portion of the coloring material that is changed becomes visible.

It is by thus comparing the slowness, with which the color of the deep tones of a gamut are weakened, and the rapidity with which that of the light tones of the same gamut disappears, that we can explain the influence of time on the tapestries of the Gobelins, and the carpet of the Savannery, in destroying the harmonies of the degradation of the colored lights and shades, and how it would be necessary, in the technical work of tapestry, and the choice of models, to take into consideration the observations that I have just made, to attenuate as much as possible, an inconvenience that we cannot completely destroy.

CHAPTER IV.—*On the observations demonstrated in the second chapter, with regard to the different nature of the stuffs, to which a similar coloring material was applied, taking care to be under the same circumstances.*

The opinion is very generally professed that wool has the most affinity for coloring matter, whilst thread (cotton, flax, and hemp) is that which has the least; and it is conformably to this opinion that it has been advanced in a memoir read at the Institute, that the object of several practical operations in dying cotton Turkish red, is to increase the affinity of the stuff for the red material of the madder, by combining with an animal material, or as it is called, *animalising* it.

The opinion which assigns to the wool an affinity for coloring materials superior to that of the thread, or even silk, rests on a system of experiments, resulting from some scattered observations which relate to two circumstances. In the one it has been observed, the wool combines more easily with the coloring materials than the thread, or even the silk; in the other, that the wool dye resists more than the thread and even silk, the light, or more generally some agents which tend to discolor these stuffs.

My observations take away all generality from this opinion, for:

In dry vacuo the light does not act on the annotto fixed to the cotton and silk, while it acts sensibly on that which is fixed to the wool.

In steam, the light changes the seaflower dye fixed to the wool and silk, in a time that the cotton which is dyed with it preserves its rose color; the only change then visible, is a tendency to violet in its coloring matter.

In steam, the light does not change the archil fixed to the wool and silk, while that which is on the cotton is discolored.

In dry vacuo, the light does not alter the sulpho-indigotic acid fixed to the silk, whilst it alters the same acid fixed to the wool and cotton.

In dry air and the atmosphere, the alteration of the acid fixed to the silk takes place, but with much less facility than that of the acid fixed to the other stuffs.

Indigo, fixed to the stuffs, presents, under the influence of light, dry air, and the atmosphere, precisely the inverse case to that of the sulpho-indigotic acid, for the former is less stable on the silk than on the cotton or wool.

CHAPTER V.—*On the observations mentioned in the second chapter, relating to light, and to the ponderable agents which have caused changes in the same coloring matter, fixed on similar stuffs, but on samples placed under the seven circumstances described above.*

1. *Action of light.*

When we look on specimens of the three stuffs dyed with indigotine, archil, and seaflower, those of cotton and silk, dyed with annotto, and silk dyed with sulpho-indigotic acid, which have been exposed to the light of the sun, in vacuo, for two years; when I say, we look on the specimens and compare them with their respective prior conditions, we are surprised at the freshness and height of the tone of their colors, when, if we call to mind what is generally said of the changeableness of sulpho-indigotic acid, archil, seaflower, and annotto, by light. As to the alteration of the Prussian blue in a white material, I shall return to that in my sixth memoir.

We might, at last, consider whether the light would not act on the indigo, archil, and seaflower, by prolonging the exposure beyond two years.

2. *Action of the light, and of dry air.*

Light causes much greater changes in dry air than when alone in vacuo; but the changes are not equally determinate in all coloring matters.

The change is but slightly sensible on the Prussian blue fixed to cotton; it is more so when it is on silk and wool.

It is but little determinate on indigo fixed to wool and cotton, but is more so when fixed to silk.

Sulpho-indigotic acid is but slightly weakened on silk, though it is, considerably, on wool and cotton.

Archil is destroyed on cotton, but leaves a reddish trace on silk and wool.

Annotto is still rather red on cotton; of a feeble and onion peel tone on silk, and completely destroyed on wool.

The yellow of the Indian saffron, and rose of the seaflower, are completely destroyed on the three materials.

3. *Action of the light, and moist air.*

Light and moist air do not produce a change on stuffs dyed with Prussian blue very sensibly greater than that of light and dry air,—it is the same on indigo fixed to wool.

It is also the same for archil and seaflower, applied to the three materials; for annotto applied to the wool and silk only, and even for Indian saffron applied to the same materials, excepting, however, that the silk dyed with Indian saffron, is of a deeper grey than the samples exposed in the dry air.

Light and moist air, on the contrary, change indigo fixed to cotton, and sulpho-indigotic acid fixed to the three materials, much more than light and dry air. The difference is most remarkable on silk and wool.

Indian saffron and annotto, fixed on cotton, under the influence of light, are much more changed in moist than in dry air.

4. *Action of light, and of the atmosphere.*

The action of light and of the atmosphere, is nearly the same as that of light and dry air, on Prussian blue, on indigo, fixed to wool, and on seaflower.

On the contrary, it is stronger on indigo fixed to cotton and silk, on sulpho-indigotic acid fixed to silk, on archil, annotto, and Indian saffron.

It is almost equal to that of light and moist air on sulpho-indigotic acid, applied to cotton and wool, on indigo, applied to cotton and silk, and on annotto.

It is stronger on archil, seaflower, and especially annotto and Indian saffron.

5. *Action of light and steam.*

Light and steam bleach Prussian blue applied to the stuffs more rapidly than light does :—it also produces a brown deposit on the vessel containing the steam, which was not the case in the vessel in which the dry vacuum was made. I shall return to this deposit in the sixth memoir.

Light and steam change Indian saffron, annotto fixed to cotton and wool, seaflower fixed to cotton and wool, archil fixed to cotton, and what is very remarkable, they only slightly weaken the rose of the seaflower fixed to cotton, and hardly at all the archil fixed to silk and wool.

6. *Action of light and hydrogen gas.*

The stuffs dyed with Indian saffron, annotto, seaflower, and archil undergo the same in dry hydrogen gas as in vacuo. It seems, therefore, that a pressure, equal to that of the atmosphere, exercised on the dyed stuffs by a gas deprived of a chemical action, has no mechanical influence to retain the gaseous elements of the stuffs, and we must add, that it has no more influence than the vacuum to alter them.

7. *Action of light, steam, and hydrogen gas.*

Light, steam, and hydrogen gas, give results almost similar to those given by light and steam.

CHAPTER VI.—*Observations mentioned in the second chapter, regarding the theory of bleaching.*

To establish the theory of bleaching stuffs, in a precise manner, requires, necessarily, two kinds of information :

1st. That which concerns the determination of the kinds of immediate principles of the stuffs to bleach ; the composition of these principles and their essential properties ;

2nd. The knowledge of the actions of the different bodies employed in bleaching the stuffs; knowledge which relates, first to the cirumstances of light, temperature, and the ponderable proportions of the reacting materials, and then to the products of these actions.

M. Chevreul considers under these two heads, the connexions of the experiments and observations mentioned in this memoir, with the theory of bleaching.

He shows, that excepting the stuff dyed with Prussian blue, we cannot, by light alone, discolor, to perfect whiteness, any of the materials he has examined.

That we can have but little hope of discoloring to white, in the air, any but cotton dyed with Indian saffron, annotto, seaflower, and archil.

M. Chevreul applies himself to some reflections on the applications of his experiments.

1st. *Regarding the proof of the stuffs dyed, and the consequences of this proof;* he gives as an example of this applicatation, the fact, that indigotic acid, so changeable on cotton and wool, is more stable on silk than even indigo.

2nd. *With regard to some phenomena which are presented by living beings, and the cause of which is attributed to light:*

He asks if air or other bodies do not intervene in these phenomena, as in those of discoloration, when a material agent is necessary."

XXXIX. *Electrical Society of London.*

Saturday, Feb. 3. A communication from Martyn Roberts, esq. was read, detailing phenomena observed in the course of experiments on the application of galvanism to the manufactures, new to him and seemingly of great interest.

A copper tube, three inches long and one inch in diameter, immersed in sea water, was connected with one end of a galvanometer: in the tube was placed a rod of zinc, three inches long and half an inch diameter, connected with the other end of the galvanometer. When the temperature of the solution of salt was 56° Fahr. the deviation of the galvanometer was 30°; temperature of solution 212°, deviation 65°. When the copper tube and zinc rod were immersed in rain water, temperature 55°, deviation was 10°; temperature 210°. deviation 23°.

A rod of iron, of the same size as the zinc, was substituted for it in the sea water, the temperature of which being 56°, the deviation was 15°; temperature 210°, deviation 41°. In this latter experiment, the progression to the extreme heat was noted. It appeared to follow the scale of an increase of one degree of deviation for every five degrees of temperature, except in the deviation at 210 degrees temperature, instead of being 45 degrees was only 41. The variation chiefly appears between the temperature of 115° and 170°, where an increase of only 3° is shown.

Whether this was the result of accident or of any cause overlooked, cannot be determined, as Mr. Roberts merely communicated facts; which, not having been before observed, might possess some claim to notice.

Also, the second of a series of papers by Mr. Sturgeon, on the various classes of electrical phenomena and their laws. Mr. Sturgeon's series of papers, when completed, will appear in the Society's Transactions.

Saturday, Feb. 17. Lieut. Morrison exhibited to the Society his electrometer, a magnetic needle suspended to a brass pointed conductor and insulated by a glass cover. It having been thought probable that its action might be thermo-electrical or hydro-electrical, and not simply electrical, it was tested by various hourly observations in December and January last, by the Meteorological Society.

An account of these observations was read to the Society, detailing the states of the thermometer, barometer, and electrometer, concluding with the opinion that the instrument was affected simply by electrical influence; that it indicated the quantity and intensity of the electric fluid present; its state and condition; and that the ratio of deflection depended on the state of electricity in the atmosphere. It was questioned in the discussion which ensued, whether the instrument acted upon the principle of electro-magnetism, and that if wood or straw were suspended in the same manner, whether they might be similarly influenced?

XL. MISCELLANEOUS ARTICLES.

Description of LIEUTENANT MORRISON'S *Magnetic Electrometer.*

This instrument consists of a bell glass open at both ends, resting, with its wide end downwards, upon a wooden sole, whose upper side is covered with a circular card, graduated similar to the card of a compass-box. The upper end of the bell glass is furnished with a brass cap, terminating upwards with a brass ball. From the lower side of the cap, hangs a fine gold wire in the axis of the glass: and to the lower extremity of the wire, and a little above the graduated card, hangs a magnetic needle. The exterior figure of the instrument is tolerably well represented by fig. 55, Plate VII. The diameter of the lower end is about four or five inches, and the height of the glass about six inches. If the reader, whilst looking at fig. 55, can imagine that the ball and its thick wire, are replaced by the magnetic needle and its thin gold wire; and that instead of the ball on the top, there is screwed into the cap, a vertical wire about three quarters of a yard long, whose upper extremity is finely pointed; we could not convey to him a better idea of the structure of the instrument.

Remarks.

It will be seen in our Report of the London Electrical Society, that on Saturday night the 17th Feb. 1838, Lieutenant Morrison brought forward his magnetic electrometer, with a view of being enabled to convince the meeting of the utility of the instrument, and of the extreme accuracy of its indications. Lieutenant Morrison's observations were prefaced by reading from the "Annals of Electricity &c." an extract from a review of Mr. Leithead's late work on electro-physiology; wherein we have stated, that "as the object of our reviews is not to find fault with observers, but, if possible, to prevent future error, we will merely say, that we have seen the instrument by which these observations were made, and are perfectly convinced that neither the motions nor positions of its needle are indicative of the electrical state of the atmosphere." See p. 78 of Vol. II. of these Annals.

In offering an opinion on the character of any instrument, we have no fear of being accused of disingenuity, nor do we suppose that our remarks on this, or any other occasion, are likely to be misunderstood. It was not our intention, whilst offering these remarks on the instrument in question, to couple with it the name of its inventor; because we could see no probability whatever, of the slightest degree of credit being derivable from its invention; and it is with very great reluctance indeed, that we are now obliged to return again to that subject, in order to convince Lieutenant Morrison, and all others who are in favour of the instrument, that our first observations are perfectly correct.

The first time of our seeing the instrument was at the Liverpool meeting of the British Association, where it was brought before the physical section. Lieutenant Morrison was asked whether it operated on the principles of electro-magnetism, or on those of common electricity; to which question no satisfactory answer was given, and no one present could see any reason why it should indicate any electric action whatever, any more than if furnished with a needle of any other material, such as wood, straw, quill, &c. The instrument was examined by Professor Lloyd, Mr. W. Snow Harris, Mr. Adams, and by some others whilst we were present; all of whom entertained precisely the same opinion respecting the inaccuracy of its indications.

When Lieutenant Morrison had introduced the subject to the meeting of the Electrical Society, he stated that satisfactory observations had been made with the instrument under the directions of the Meteorological Society, who had directed that a register of its indications should be taken and a report made thereon. This had been done, and the report was read before the Meteorological Society, on the 13th of the present month. The Meteorological Society, however, as well as Lieutenant Morrison, were desirous of having the opinion of the Electrical Society, concerning the probable utility of the instrument, and in order that the Electrical Society might be properly acquainted with what the Meteorological Society had done, the Secretary of the latter had brought with him the report of the observations alluded to, which the Chairman of the Electrical Society permitted to be read before the meeting. The following is the Report.

Report of Experimental Observations made with Two Electrometers, invented by LIEUT. MORRISON, R. N. *By* ROBERT CARR WOODS, *Instrument Maker to the Meteorological Society,* 47, *Hatton Garden, Holborn, London.*

Read at the Meteorological Society, 13th February, 1838.

In consequence of an observation that was made from the results of my thirty-seven hourly observations at the winter solstice, that the deflection of the needle became more manifest on an increase of temperature, Dr. Birkbeck remarked, that if such was the case, the gold thread by which the needle was suspended must have been influenced by torsion, and the instrument consequently became a *thermo-electric,* and not *entirely electrical.* Sir John Ross, and other members then present, were of the same opinion; and in the course of the discussion Sir John suggested, that if such was the case, the needle might be suspended in such a manner as to prevent the influence of thermometrical agency. It was, however, recommended that simultaneous observations be made with two instruments. Accordingly, being kindly furnished with two of these delicate instruments, by Mr. W. H. White, I proceeded to make observations for ten hours successively, on the 8th of January, 1838, placing one electrometer thirty inches from the ground, and the other two inches above the surface of the earth. Beside each was placed a delicate mercurial thermometer, whose exact correspondence I had previously determined; the result was as follows:

Hours of Obs.	Elect. 30 inches from the ground	Ther.	Elect. 2 inches from the ground.	Ther.
9 A. M.	3 deg. plus	31°	....3 deg. plus	30°
10	7 deg. plus	31	....4 deg. plus	30
11	3 deg. plus	31	0	30
12	3 deg. plus	31	0	31
1 P. M.	2 deg. plus	30	....2 deg. plus	29
2	2 deg. plus	31	....2 deg. plus	30
3	1 deg. minus	31	....1 deg. minus	30
4	0	31	0	30
5	3 deg. plus	30	....3 deg. plus	28
6	2	30	....2 deg. plus	28

By the above table it will be seen that the electrometer thirty inches from the ground, deflected 3, 7, 3, 3, 2, 1, 0, at the *same* degree of temperature; the *maximum* of deflection being 7 deg. *plus*, and the *minimum* 1 deg. *minus*; while the other instrument, two inches from the ground, and the thermometer 1 deg. lower than the former, deflected 3, 4, 0, 2, 1, 0, the *muximum* being 4 deg. *plus*, and the *minimum* 1 deg. *minus*. During the *two last* observations the deflection of the needle was the same in both, although the temperature in the locality of the latter was 2 deg. lower than in the former; and when the deflection was the same in the two places of observation, viz. at 9 A. M., and 1, 2, 3, 4, 5, and 6 P. M. the differences of temperature were as 31:30, 30:29, 31:30, 31:30, 31:30, 30:28, 30:28.

On the 9th of January, 1838, the needle deflected 5 deg. plus, at 9 A. M., the thermometer being then 24° Fah.; and for four successive hours the needle and thermometer remained the same without the least alteration whatever. From these and similar observations, I think I may conclude, that the electrometer invented by Lieutenant Morrison is *not* a *ther-*

mo-electric. The improvements suggested by Sir John Ross I think will be valuable; for the observations with the instrument, in its present form, is attended with considerable trouble, and, perhaps, inaccuracy, from its oscillatory motion, on receiving currents of air, &c. To-day (January 9th), I heaped round the glass bell a quantity of snow, but no deflection took place in consequence. The *maximum* of atmospheric electricity was to-day 8 deg. plus, at 4 P. M., barometer 30·115, attached thermometer 39°, external thermometer 22°·5, wind N. E., phenomena, snow.

I at first thought the electrometer might be *hygro-electric*, but I am convinced, as far as my observations and experiments have extended, that it is simply an *electrical instrument*, and with the necessary improvements, it will be of great assistance in meteorological phenomena.

London, January 9th 1838.

Observations for twenty-one hourly observations made with one of Lieutenant Morison's electrometers, on the 24th and 27th of December, 1837. By W. H. White, Secretary to the Meteorological Society.

Dec. 24th.

Hour of obs.	Deflection.	Wind.	Ther.	Weather.
9 A. M.	88° +	S. W.	48·0°	clear
10	85 —	gusty	49·9	mist falling
11	5 +	S. W.	50·0	clear
12	10 +		51·0	do.
1 P. M.	90 +		52·5	dull
2	30 +		53·0	light deps.
3	42 +	S. S. W.	53·5	clear
4	96 +		52·0	strong breeze

From 9 till 10 A. M. the magnet was almost in constant motion. At a quarter before 10, the magnet suddenly deflected to 175°+, and in ten minutes afterwards returned to 85°—, wind in gusts with some deposition. After 10 the air became more settled, and the variations all indicated a positive state of electricity.

Dec. 25th.

Hour of obs.	Deflection.	Wind.	Ther.	Weather.
9 A. M.	10° +	S. S. W.	52·5°	clear
10	10 —		53·0	light breeze
11	18 —	W.	53·0	strong breeze
12	40 —		55·5	do.

Hour of obs.	Deflection.	Wind.	Temp.	Weather.
1 P. M.	40 +	W.	56·0	do.
2	50 —		55·5	do.
3	50 —		55·0	wind abated
4	40 —		53·0	do. strengthened
5	40 —		52·0	do.
6	40 —		51·0	light wind
7	30 —		50·0	nearly calm
8	20 —		49·0	calm
11	10 +		47·0	overcast

The character of the weather to-day was more settled and summer-like, and the magnet indicated a negative state of the electric current, except the first and last observations.

Obs.—The electrometer was placed on the window sill, about two feet from one of Crichton's thermometers. Aspect, W.

Observations for twenty-three successive hours, copied from the thirty-seven hourly observations at the winter solstice, (1837), made by Mr. Robert Carr Woods, for the Meteorological Society, London.

		Bar.	Ext. Ther.	Wind.	Electr.
Dec. 21	8 P. M.	30·100	41·75	E. N. E.	5° plus.
	9	·120	41·75	E. N. E.	3
	10	·130	41·25	E.	3
	11	·130	42·00	E. N. E.	3
	12	·130	41·50	E. N. E.	3
Dec. 22	1 A. M.	·120	41·75	E. N. E.	5
	2	·110	40·35	N. E.	7
	3	·112	40·55	N. E.	7
	4	·070	40·25	N. E.	7
	5	·050	40·25	N. E.	8
	6	·030	42·25	N. E.	8
	7	·032	43·00	E. N. E. var.	8
	8	·022	43 25	S.W.	8
	9	·005	45·00	S.W.	8
	10	30.000	46·00	W. S. W.	9
	11	29·960	47·50	W. S. W.	10
	12	·945	47·30	S. S. W	10
	1 P. M.	·937	48·00	S. W. by W.	14
	2	·937	48·00	S. S. W.	12
	3	·900	49·50	S. W.	11
	4	·888	50·00	S. W.	12
	5	·885	51·10	S. W.	13
	6	·885	51.25	S·W.	26

At 9 P. M. the Electrometer deflected 38 deg. + External Thermometer, 57·50.

As the above report of observations was brought before the Electrical Society as a criterion of the indications of the instrument, there can be no doubt of there having been great confidence placed in them, both by Lieut. Morrison and the Meteorological Society: and as we are of opinion that the strictest candour has been observed in the drawing up of this report, we feel ourselves the more inclined to pay even more attention to it than those connected with it could perhaps have expected.

We do not know what were the suggestions of Sir John Ross, and therefore have no means of judging whether the instrument would be improved by them or not; but whoever is interested in knowing the present condition of the instrument, ought, certainly, to be obliged to Mr. Carr Woods for his candour in stating that "the observations with the instrument in its present form, are attended with considerable trouble, and perhaps, inaccuracy, from its receiving currents of air, &c." We are also of opinion that the observer has made one very just conclusion, viz. that the instrument *is not a thermo-electric one;* for the needle varied 8° at one and the same degree of temperature during the first day's work; and on the second day, January 9th, when the temperature had fallen 7°, the needle was still deflected as far as on the previous day.

We now understand by Mr. Woods's observations, that the instrument does not operate upon the principles of heat; and if nothing had been said about the agitation of its needle by the wind, we might possibly have been at a loss to make even a guess respecting the cause of its motions. Mr. Woods's opinion that the instrument operates as an "*electrical instrument,*" may possibly be very correct; but it would certainly have been more satisfactory had it been accompanied with the *reasons* on which it is founded.

Having disposed of Mr. Woods's part of the Report, we pass on to that part of it which appertains to Mr. White's observations, which were made on the 24th and 25th of December, 1837. In the first day's work, we perceive something similar to that which is exhibited in Mr. Woods's observations. There is *no correspondence* in the deflections and the temperature, but much agitation of the needle during the wind. The needle being driven through an arc of 260°, at a quarter before 10, was an *excellent indication* of—we will not say what; but one cannot help looking at the word "gusty," so conspicuously placed in the table.

The table of observations for the 25th of December, again shows the total disrespect which the needle evinced for difference of temperature, and something like a proneness to obey the impulses of the wind. At 9 in the morning, when the wind was asleep, the needle stood 10° out of the meridian; but the moment the "light breeze" commenced, the deflection became 10° in the other direction. As the breeze grew stronger, the deflection got greater, and before the wind abated, the deflection arrived at 50°. This happened from 2 to 3 o'clock in the afternoon. From that time the wind gradually abated, so did the deflections of the needle; and when there again became a dead calm, at 11 o'clock at night, the needle was found to have receded to precisely the same place that it stood at during the calm in the morning.

During this series of observations, there seems to have been as precise a coincidence in the operations of the wind and indications of the needle, as any one need to have wished for: the whole *movement* corroborating in the most satisfactory manner, Mr. Woods's observations regarding the effects which "currents of air" had on the needle of the instrument.

The inferences which we have drawn from these observations may, perhaps, be considered very unfair, when it is seen that the observers have invariably arranged the whole of the deflections of the needle in electrical language; implying thereby their entire confidence in the accuracy of its indications, as an atmospheric electrical instrument. Under these circumstances our readers may possibly require of us some explanation respecting the cause of our forming so very different an opinion.

In complying with such a request, we should, in the first place, be led to enquire of the inventor and observers, how they became acquainted with the *character* of the indications of the instrument? By what means has it been made known to them that the deflections are due to electricity? From what reasoning, from what experiments, or by what criterion is the instrument known to be an electrometer, or even an electroscope? This is not the first time that these enquiries have been made. They were made at the Liverpool meeting of the British Association, and repeated at the meeting of the Electrical Society: but as yet they have never been favourably answered; but, on the contrary, Lieut. Morrison has very candidly stated, at both places, that he is not acquainted with the principles upon which the instrument operates. Perhaps a similar answer may be expected from the observers; at least, we are in no fear of being disappointed by anticipating such a one. The concession, however, might possibly be considered as no just basis on which to form a correct notion of the real character of the instrument: and therefore it is essential that we state, from our own knowledge, that there is *no known principle* in any department of electricity whatever, nor any known law of electric action, by which its deflections could be recognized as indicating the electric condition of the atmosphere. No electrician will attempt to deny this truth; and we are regardless about what others say.

We should not have again returned to Mr. White's observations, had he not happened to mention that one set of them, at least, were made on the sill of a window; but in consequence of becoming acquainted with that fact, we discover a necessity of warning observers not to trust to atmospherical electric observations, made in such situations. They could be but of little value, even with a good electrometer: and, with the instrument in question, much deception might occur from local forces perfectly concealed from many observers. There are two mischievous pieces of iron, sometimes four of them, suspended from the ends of the sash lines, which operate pretty forcibly on a magnetic needle. We simply mention this fact, without taking upon ourselves to say whether such forces had, or had not, to do with Mr. White's observations. Whilst on this topic, however, we will mention another place of concealment, from which these ambuscaders are likely to have produced occasional mischief.

Nocturnal magnetic observations must necessarily be attended with an artificial light. Perhaps no one on such an occasion would approach a magnetic needle with an iron candlestick; although they would not scruple to take a silver one. The rogue is in the silver candlestick. The slider and spring are usually of iron or steel. If Mr. White should discover that such forces have influenced his observations, it may be gratifying to him to know that he is not singular in this respect.

It will be seen in the quotation from Mr. Leithead's book, at page 78 of this volume of the "Annals," that the *easterly* and *westerly* deflections of the needle in Lieutenant Morrison's instrument are, respectively, the *supposed* indications of the *positive* and *negative* electric conditions of the atmosphere. Now, in presenting a novel instrument to the notice of scientific men, we would certainly have expected, that the inventor would be prepared to show by what means he was led to such a conclusion. On this important point, however, no satisfactory information has been given: nor is any likely to appear. It might now be asked, by what means do the observers adjust the instrument to the magnetic meridian? Here again is wanting another piece of important information. The instrument appears by the tables, to have been under the influence of electric action during the whole time of observation, and for want of being made acquainted with the *means* which the observers had to discover the magnetic meridian, we are at a loss to know *how* they ascertained the true bearing of the needle. The needle of the instrument alone could give no information on this point, because it was continually deflected out of its true position.

Have any of the observers, who have used this instrument, tried to take away the *supposed* electric influence from the needle by touching the rod with the hand, or a piece of metal, or by any other means? In short, have any observations been made with the instrument whilst its needle was uninsulated? We have not heard that the instrument has undergone, even one, of the several necessary tests we have had occasion to notice.

We hope now, that Lieutenant Morrison will see clearly that we have given his instrument a fair and impartial examination, and that our first opinion of it, was not hastily formed. The Report of the Meteorological Society, on the observations made by their directions, being supposed to be favourable to the instrument, we could have no hesitation in placing that report before our readers: and we have made no other remarks on it than such as appeared essential to place the observations in a proper light. We will now take leave of this instrument, hoping that, should we ever have occasion to speak of it again, the suggestions which we have already made, will have been duly attended to. In its present condition, we are still "perfectly convinced that neither the motions nor positions of its needle are indicative of the electric state of the atmosphere."

EDIT.

On the solution of Caoutchouc.

Much attention has been bestowed upon this article, with a view of discovering some solvent or mode of reducing it to a consistence capable of receiving any desirable form, or of being applied to the surface of cloth in the form of varnish, in order to render it waterproof: but believing that no method has yet been made public by which it could be used with economy and facility, I am induced to offer the following, with the hope that it will be found both useful and interesting.

I wish to premise, that all hitherto known solvents of *caoutchouc* are liable to objections. In a trial which I once made, I found that oil of turpentine dissolved *raw* caoutchouc tardily; and on having been spread on calico and exposed to the atmosphere, it remained glutinous at the end of a year.

About two years ago, I was induced to perform some experiments with caoutchouc, and I accidentally ascertained, that if it be previously cut fine and immersed in common sulphuric ether, or a solution of some alkali (I used carbonate of soda, 2 oz. to a pint of water) for a week, and then put into *good new* oil of turpentine, it dissolved with facility; and when spread on cloth and exposed to a dry atmosphere, it *speedily dries and assumes its original properties,* usually in twenty-four hours.

Calico, linen, or articles of clothing, may receive a coating with this solution, sufficient to render them waterproof without materially altering their general appearance or injuring their pliability.

When less elasticity and more body is required, I hazard a conjecture that this solution may economically be diluted or mixed with asphaltum, Venice turpentine, or some other articles soluble in oil of turpentine.

Meriden Ct. Nov. 29, 1837. ARZA ANDREWS.

Silliman's Journal.

Disengagement of Light during the Chemical combination of Metals. By M. VOGEL, *of Munich.*

Electro negative metals, which form with oxygen peculiar oxide, like arsenic and antimony, are very prone to combine in definite proportions with electro-positive metals, whose oxides constitute saline basis.

These combinations of positive with negative metals, are distinguished in various ways from simple alloys, but principally in their not separating when melted and kept long together in a state of fusion. The heavier does not go to the bottom, nor the lighter to the surface. We know on the other hand, that in an alloy of gold, silver and copper, in quiet fusion together, the gold is found in greater quantity at the bottom than at the top of the crucible; in like manner in bell metal, the tin separates in part from the copper.

Zinc and tellurium combine with a considerable disengagement of caloric. In making brass, the melted copper and zinc, at the moment of their union, are apt to spring out, (jetés en dehors) but whether light is disengaged or not, we cannot tell, because the melted mass is itself at a red heat.

The analogy which exists between arsenic and phosphorus is generally known; but arsenic in its relation to the metals, acts like sulphur; at least, with respect to the electro-negative metals, it conducts exactly like sulphur itself, as the following experiment proves.

In a covered earthen crucible I melted two atoms of zinc (64·2) and withdrew the crucible from the fire, and then I added an atom of metallic arsenic (37·6) in fine powder, but previously heated to the point of boiling water. In stirring the mixture with a porcelain rod, the mass—which before was far from being luminous,—became incandescent and of a deep cherry red, just as when sulphur unites with different metals; and at this time a small quantity of arsenic inflamed on the surface, and burnt with a bluish light.

It is possible that lead and tin with arsenic, in large quantities, would produce ignition. I did not succeed in producing it, nor by adding antimony to zinc, lead, or tin.

When hydrochloric acid is poured on a compound of arsenic and zinc, of equal proportions, arsenical hydrogen gas is disengaged, and is entirely absorbed by a solution of sulphate of copper.

When this gas is exposed in a flask to the sun's light, dark laminæ, more or less brilliant, cover the interior of the flask. In a dark place only a few spots appeared at the end of a week.

The sun's rays then have the same decomposing action on this gas as heat, for M. Souheiran has shown that the gas heated by a spirit lamp, deposits arsenic, increases its volume one half, and becomes pure hydrogen. We have then an analogy between arseniuretted hydrogen gas and phosphuretted hydrogen gas: for the latter exposed to the sun deposits much red phosphorus, and loses its property of spontaneous inflammation.

Jour. de Pharmacie.

Franklin Journal.

Meteoric Iron. 1. *In Texas.*

In Vol. VIII. p. 218, of this Journal (Silliman's) is an account of the great mass of meteoric iron from the Red River, now in the cabinet of Yale College. Among almost forgotten files we find a letter, dated Sparta, Tennessee, Sept. 15, and another dated October 17, 1829, from Robert Cox, to the Editor, containing the following statements.—

A gentleman returned from a five years' absence in the province of Texas, during which time he had been frequently with the Camanche Indians, and a small party of them conducted him to a mass of metal lying on the bank of a creek. Its length was four feet, and it was about one foot square (at the end). It required six of the Indians to raise it on end. A piece weighing two ounces was cut off by a tomahawk. It possessed great hardness and tenacity, and when hammered, (in the cold) showed great malleability, being easily beaten out very thin without cracking or scaling. The colour was stated to be between that of gold and silver. Its lustre was remarkable, and could not be tarnished by any thing that was done to it, even by the application of heat. The large mass of metal seemed to defy every attempt to make an impression on it, except under the hammer, when it became pliable and soft. From the acquaintance which we have with the large mass alluded to above, we cannot doubt that the piece described in Mr. Cox's letter is nickeliferous meteoric iron. Those that saw the piece were disposed to make it out to be gold, and probably saw a yellow tint quite as strongly as it existed, if indeed it existed at all, for the malleable iron which we have from the same region is like that of Siberia, of a remarkably pure greyish white, with a high degree of lustre.

We have recently seen a gentleman, who stated that he knew of several large pieces of malleable iron in Texas, and we hope to obtain some more precise information concerning them.

2. *Meteoric Iron in France.*

The late Colonel George Gibbs brought to this country some pieces of meteoric iron which he detached from a large mass lying on the mountains of Auvergne in France, and a notice of it was published in Dr. Bruce's Journal of Mineralogy, in connexion with one of the Louisiana iron.

The following extract is taken from a letter addressed to the editor, by Mr. Wm. C. Woodbridge, the well-known geographer, and dated Paris, August 29, 1829:—

"In passing through Bonn, upon the Rhine, I visited Professor Noeggeratti, a distinguished mineralogist of that university. He spoke with great interest of our efforts in reference to mineralogy, and especially of the American Journal. He observed to me that, singular as it was, he had received through that Journal, the first account of an interesting fact in his own neighbourhood.

"He had heard many years since of a large mass of iron lying on one of the mountains termed 'the Seven Mountains,' in this vicinity, but which was supposed to be a remnant of an old furnace. He designed to examine it, but delayed from time to time, and at length heard that a foreign officer had been there, and taken away a large portion. He thought little more of it, until some time after, when he saw in the American Journal of Science, Col. Gibbs's account of his discovery of a mass of meteoric iron on this very spot. He immediately went to examine the fact; he found that the mass had been cut up and put into the forge, but the smiths not having skill to work it, it was again thrown aside, and lay buried under a heap of scoria. Professor N. after some search, discovered a very large quantity of this iron, and verified the existence of nickel, and the truth of the account which the American Journal had been the medium of announcing to the world, of one of the largest masses of meteoric iron yet discovered."

Silliman's Journal, January, 1838.

Professor Silliman thinks that the account here referred to, must have been that originally published in Dr. Bruce's Journal.

On the boiling points of saturated solutions of various Salts. *By* M. Legrand.

The following table indicates the number of degrees of the centigrade thermometer which the several solutions require above that of boiling water, when pure.

Salts.			Prop of salt to 100 of water at the point of saturation.	Degrees above pure boiling water.
Chlorate of Potash,	-		61·5—	4°·2 cent.
Chloride of Barium,	-	-	60·1—	4·4
Carbonate of Soda,	-	-	48·5—	4·6
Chloride of Potassium,	-	-	59·4—	8.3
Chloride of Sodium.	-	-	41·2—	8·4
Hydrochlorate of Ammonia		-	88·9—	14·2
Tartrate of Potash (neutral,)		-	296·2—	14·6
Nitrate of Potash,	-	-	335·1—	15·9
Chloride of Strontium,	-	-	117·5—	17·8
Nitrate of Soda,	-	-	224·8—	21·0
Carbonate of Potash,	-	-	205·0 –	35·0
Nitrate of Lime,	-	-	362·2—	51·0
Chloride of Calcium.	-	-	325·0—	79·5

A solution may be supersaturated, notwithstanding the commotion of boiling, and attain a temperature more and more elevated, but as soon as the salt subsides, the thermometer goes down again to the point at which it remains during the whole of the evaporation. The liquor is then simply saturated.

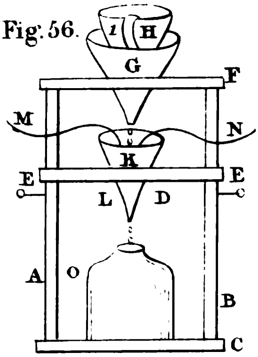
Some salts, even in small proportions, prevent the jumping (soubresauts) of liquids during ebullition; others, on the contrary, and particularly carbonate of potash, promote it in a high degree. Various metallic filings prevent these jumpings or bouncings, but all do not possess the property in an equal degree. The most oxidable, as iron or zinc, are the most efficacious. They are not changed during the operation.

Franklin Journal. *Annales de Chemie.*

ANNALS OF ELECTRICITY, &c.

PLATE. VIII.

Fig. 56.



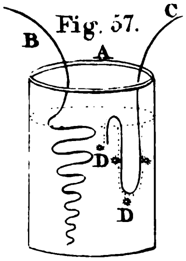


Fig. 57.

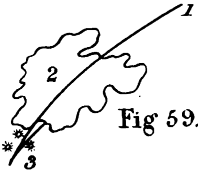


Fig 59.

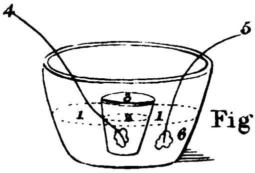
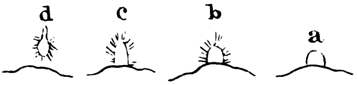


Fig. 58.



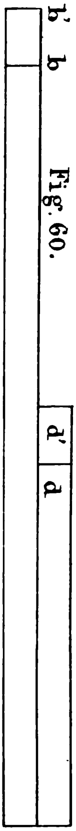


Fig. 60.

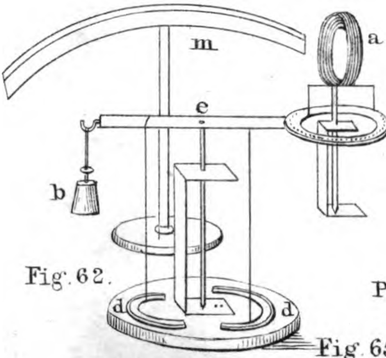


Fig. 62.

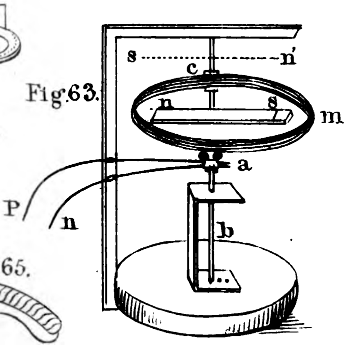


Fig. 63.

Fig. 65.

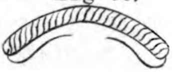
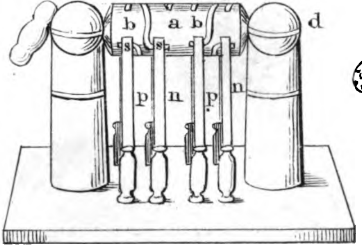


Fig. 64.



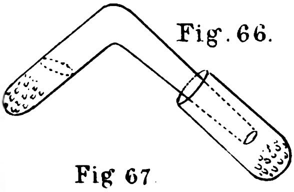
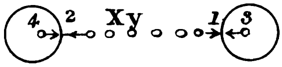


Fig. 66.

Fig 67



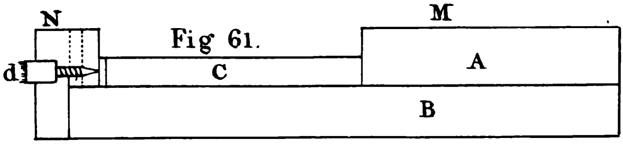


Fig 61.

W. Annan. lith: 12 Gracechurch St.

THE ANNALS
OF
ELECTRICITY, MAGNETISM,
AND CHEMISTRY;
AND
Guardian of Experimental Science.

APRIL, 1838.

XLI. *Reply to Mr. Snow Harris's paper on Lightning Conductors, by* MARTYN ROBERTS, ESQ.

To the Editor of the Annals of Electricity, &c.

Dear Sir,

By the negligence of my Bookseller, it was only yesterday I received No. 8, of your Annals, containing Mr. W. S. Harris's reply to a paper on lighting conductors, read by me before the London Electrical Society.

I am surprised to find Mr. Harris's letter written with a feeling of so much asperity, more especially as whenever I have alluded to that gentleman, it has always been with marked courtesy, and I am sorry to perceive that any warmth of feeling has been mixed up with scientific arguments on a question of so much importance, and one which demands a calm and patient discussion.

The report quoted by Mr. Harris is certainly much in his favour, if there is no doubt that the lightning passed through the ship; but the fact of the tremulous motion of the vessel, which is brought forward to prove such a transmission of the fluid, might have been occasioned by the vibration of the air from the loud peal of thunder: but granting the lightning to have passed through the ship, if it can be proved on scientific principles that the situation proposed by Mr. Harris, for conductors, is dangerous, one occurrence such as that on board the Beagle, ought not to warrant the exposing other ships to the chances of a similar escape.

Mr. Harris imagines I depreciate his invention for the purpose of bringing forward my own; my object was not personal but national benefit; and if I had condemned Mr.

Harris's conductor, without suggesting a better form, he might then have had cause to complain that I found fault, being at the same time unable to propose a plan for amendment.

Mr Harris says he has "*nowhere maintained that superficies not content conducts electricity.*" To refute this assertion I give an extract from Mr. Harris's work on Electrical Conductors, page 31. "*The conducting power of a metallic rod has but little relation to its solid contents, but is principally dependant on its surface, from which cause the mere gilding of a ball of wood is found to conduct a proportionate electrical discharge with the same facility as if such ball was a solid mass of metal; hence a less quantity of metal formed into a hollow tube would be as a conductor, even more effectual than a solid rod of the same diameter, because its* SUPERFICIES *would be increased.*"

After this I hope Mr. Harris will have the candour to confess he has maintained that "superficies not content conducts electricity."

In the next place Mr. Harris blames the indefinite manner in which I describe my wire rope, viz., as one of some hundreds of annealed copper wires; but he forgets that my paper was read before a Society at the same time that a specimen of the full sized rope was exhibited; Mr. Harris imagines it could not be considerable in consequence of its great weight. Now, it appears to me an efficient quantity of metal will be of the same weight, whether in the form of Mr. Harris's plates or in that of my wire rope. This objection, therefore, (if it is one) is equally applicable to Mr. Harris's invention.

This gentleman then goes on to say "he does not himself believe that even if there did occur a short interval in the caps, that it would be of the least consequence to the action of such an extended and continuous line of metal, armed with a point such as he employs." I am surprised Mr. Harris should maintain such an opinion, and I will adduce first rate authority in support of a contrary one; for M. Becquerel, one of the most eminent electricians of the present day, has thus expressed himself, in page 141, Vol. IV. of his work on Electricity. "Nous repetons encore que le parratonnere doit être en relation parfaite avec un sol humide, car lorsque son conducteur offre *quelque part* des solutions des contenuité il arrive que la fondre apres l'avoir frappé l'abandonne pour se porter sur un corps voisin qui lui offre plus de facilité pour se rendre dans le sol ce changement de conducteur *est presque toujours accompagné d'explosion et de degats plus ou moins graves.*" Which is to say that when a conductor *has in any* *part a solution of continuity* the lightning abandons it and strikes on some neighbouring body, and that this change of conductor is almost always accompanied by explosion and by damage more or less serious. This opinion is supported by a committee of gentlemen deputed by the French Academy of Science, to report on the best form of conductor.

But the most important admission made by Mr. Harris is, that he denies the existence of the lateral explosion.

Now, as my name is but little known in the scientific world, I will not bring forward my own experiments; but I will quote the authority of a gentlemen, to which, I am sure even Mr. Harris will pay respect; I allude to Professor Henry, who, at the Meeting of the British Association of Science held at Liverpool, stated, "That a metallic conductor intimately connected with the earth at one end, does not silently conduct the electricity thrown in sparks on the other end. In one experiment a copper wire one eighth of an inch in diameter was plunged at its lower end into the water of a deep well, so as to form as perfect a connexion with the earth as possible, a small ball being attached to the upper end, and sparks passed on to this from a machine. *A lateral spark could be drawn* from any part of the wire, and a pistol of Volta fired even near the surface of the water. This effect was rendered still more striking by attaching a ball to the middle of the perpendicular part of a lightning rod, put up according to the directions given by Gay Lussac, when sparks of about an inch and a half in length were thrown on the ball; corresponding *lateral sparks* could be drawn not only from the parts of the rod between the ground and the ball, but from the part above even to the top of the rod."

You perceive, sir, I have the opinion of an eminent scientific gentleman in support of the position I have maintained, viz., that the lateral explosion occasioned by a transmission of electricity is sufficient to produce ignition, and this even with the feeble quantity of the fluid placed at our command by electrical machines, and it is the more conclusive, as the experiment was tried on a lightning conductor erected on the most efficient plan then known: but if Mr. Harris chooses to deny that Professor Henry has witnessed this phenomena, of course there is an end to all argument on the question.

Mr. Harris then proceeds to state that, "admitting the objection however of the lateral explosion to be a valid one, it necessarily applies equally, if not more forcibly, to Mr. Roberts's rope of wire than to my plates of copper, as I think must be apparent, since the wire is directed to be laid along the back of the mast and stopped to the rigging."

It is singular Mr. Harris should attempt to disparage my invention without having carefully read the description I have given of my conductor. I cannot suppose any gentleman would intentionally convey a false impression of its merits, and if Mr. Harris will refer to my first paper on this subject* he will find that I do *not* direct the conductor to be stopped to the rigging, but it shows how it must be set up independently as a back stay from the lower mast head to the sheathing, far removed from any "tarred rope or sheet of canvass."

From the royal mast head to the lower mast head, my conductor pursues nearly the same course as that of Mr. Harris; for along the higher mast there is comparatively little danger to be apprehended from the lateral explosion: but the case is far different when we arrive at the hull. This is the real place of danger, and here Mr. Harris's conductor is led; whereas, mine on the contrary is carried from the lower mast head to the sheathing, thereby avoiding all danger to the hull and its contents. But as Mr. Harris's objection is evidently based on a misconception of my plan, I need not pursue his argument further.

Mr. Harris then brings forward the fact that conductors are placed near powder magazines on shore, and argues therefrom that no danger can arise from the contiguity of conductors to powder magazines at sea, but Mr. Harris should remember that on shore the conductors are detached from the building, and not contiguous to any body upon which the passing current can throw its lateral spark to the ignition of the powder; but at sea, if a conductor is led through the hull and near the magazine, it will be surrounded by bolts and other metallic bodies peculiarly fitted to elicit the lateral spark.

Mr. Harris then goes on to show that no action is visible when electricity traverses a chain, but he ought to know, that in passing a charge through a chain a spark is seen at every junction of one link with another; a phenomenon which can be proved to exist by any one possessing the ordinary electrical apparatus.

Mr. Harris then proceeds (he says "not with an unfriendly feeling," but with what other I am at a loss to divine) to state that I have failed to fulfil my profession of examining the causes of accidents at sea, from lightning &c. Now the fact is, I have expressly stated "the causes can be traced to two principal heads, viz., the form and position of the conductor." It is true I have not given a long high sounding list of accidents, for unfortunately they are too well known to require a recapitulation, and I little thought it would be necessary to impress on my hearers that accidents had occurred from lightning.

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* Annals of Electricity, &c., Vol. I., page 468.

Again, Mr. Harris finds a want of originality in my investigations, but I think I may safely assert that prior to the appearance of my paper no form or position was proposed for a conductor, that would obviate the objection of the lateral explosion, nor am I aware this objection was ever before raised to conductors then in use.

And further Mr. Harris states "I have substituted objections not warranted by any known fact." Here he is in error, for, as I have before shown, Professor Henry has proved the fact of the lateral explosion in a most satisfactory manner, and I may also call to my aid the statement of another distinguished philosopher, Dr. Roget; who has shown that a shock *is* felt on grasping a wire, through which a charge is passed; and he has also instanced another experiment in which the lateral explosion is apparent; therefore, unless Mr. Harris proves that Dr. Roget, Professor Henry, and M. Becquerel have stated what is not the fact, I can decidedly contradict Mr. Harris when he states I have brought forward objections "unwarranted by any known fact."

In reply to the last paragraph of Mr. Harris's paper I assert that my rope does not require constant watching, and that it is quite as able "to take care of itself" as any series of plates can be, and that neither the standing nor running rigging is at all interfered with, and that a perfect continuity of conduction is maintained under all circumstances.

I do not doubt but that Mr. Harris has adhered to conclusions drawn from his own experiments; but for the support of my position I have not entirely relied upon my own experience, but have brought forward to my aid the opinions of three distinguished philosophers.

I am very sorry, if in this communication I have not maintained the strict courtesy due from one gentleman to another, for it has been my wish and object so to do, and I hope I may never be betrayed into any warmth of expression unsuitable to the dispassionate manner in which scientific subjects should be treated.

Remaining, Dear Sir,
Truly yours,
MARTYN ROBERTS.

Bryn. y. caerau, Llanelly,
Carmarthenshire,
March 8th, 1838.

XLII. *Description of some Experiments made with the Voltaic Battery, by* Andrew Crosse, Esq. *of Broomfield, near Taunton, for the purpose of producing Crystals; in the process of which Experiments certain Insects constantly appeared. Communicated in a Letter dated 27th December,* 1837, *addressed to the Secretary of the London Electrical Society.*

Read 20th January, 1838.*

My dear Sir,

I trust that the gentlemen who compose the "Electrical Society" will not imagine that because I have so long delayed answering their request, to furnish the Society through you, as its organ, with a full account of my electrical experiments, in which a certain insect made its unexpected appearance, that such delay has been occasioned by any desire of withholding what I have to state from the Society in particular, or the public at large. I am delighted to find that at last, late, though not the less called for, a body of scientific gentlemen have linked themselves together for the sake of exploring and making public those mysteries, which hitherto, under a variety of names, and ascribed to all causes but the true one, have eluded the grasp of men of research, and served to perplex, perhaps, rather than to afford sufficient data to theorise upon. It is true that much has been done in the course of a few years, and that which has been done only affords the strongest reason for believing that vastly more remains to be done. It would be presumptuous in me to enumerate the services of a Davy, a Faraday, and many other great men at home, or a Volta and an Ampère, with a host of others abroad. These distinguished men have laid the foundations, on which their successors ought to endeavour to erect a building worthy of the scale in which it has been commenced. Electricity is no longer the paltry confined science which it was once fancied to be, making its appearance only from the friction of glass or wax, employed in childish purposes, serving as a trick for the school boy, or a nostrum for the quack. But it is, even now, though in its infancy, proved to be most intimately connected with all operations in chemistry, with magnetism, with light and caloric; apparently a property belonging to all matter, perhaps ranging through all space, from sun to sun, from planet to planet, and not improbably the secondary cause of every change in the animal, mineral, vegetable, and gaseous systems. It is to determine whether this be or not, the case, as far as human faculties can determine, to ascertain what rank in the tree of science electricity is to hold; to endeavour to find out to what useful purposes it might be applied, that I conceive is the object of your Society, and I shall at all times be ready and willing, as a member, to contribute my quota of information to its support, knowing well, that however little it might be, it will be as kindly received as it is humbly offered. It is most unpleasing to my feelings to glance at myself as an individual, but I have met with so much virulence and abuse, so much calumny and misrepresentation, in consequence of the experiments which I am about to detail, and which it seems in this ***nineteenth century*** a crime to have made, that I must state, not for the sake of myself (for I utterly scorn all such misrepresentations), but for the sake of truth and the science which I follow, that I am neither an "Atheist," nor a "Materialist," nor a "self imagined creator," but a humble and lowly reverencer of that Great Being, whose laws my accusers seem wholly to have lost sight of. More than this, it is my conviction, that science is only valuable as a mean to a greater end. I can assure you, sir, that I attach no particular value to any experiment that I have made, and that my feelings and habits are much more of a retiring than an obtruding character; and I care not if what I have done be entirely overthrown, if truth be elicited. The following is a plain and correct account of the experiments alluded to.

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* From the Transactions of the London Electrical Society.

In the course of my endeavours to form artificial minerals by a long-continued electric action on fluids holding in solution such substances as were necessary to my purpose, I had recourse to every variety of contrivance which I could think of, so that, on the one hand, I might be enabled to keep up a never-failing electrical current of greater or less intensity, or quantity, or both, as the case seemed to require; and on the other hand, that the solutions made use of should be exposed to the electric action in the manner best calculated to effect the object in view. Amongst other contrivances, I constructed a wooden frame, of about two feet in height, consisting of four legs proceeding from a shelf at the bottom supporting another at the top, and containing a third in the middle. Each of these shelves was about seven inches square. The upper one was pierced with an aperture, in which was fixed a funnel of Wedgewood ware, within which rested a quart basin on a circular piece of mahogany placed within the funnel. When this basin was filled with a fluid, a strip of flannel wetted with the same, was suspended over the edge of the basin and inside the funnel which, acting as a syphon, conveyed the fluid out of the basin, through the funnel, in successive drops. The middle shelf of the frame was likewise pierced with an aperture, in which was fixed a smaller funnel of glass, which supported a piece of somewhat porous red oxide of iron from Vesuvius, immediately under the dropping of the upper funnel. This stone was kept constantly electrified by means of two platina wires on either side of it, connected with the poles of a Voltaic battery of nineteen pairs of five-inch zinc and copper single plates, in two porcelain troughs, the cells of which were filled at first with water and $\frac{1}{100}$ of hydrochloric acid, but afterwards with water alone. I may here state, that in all my subsequent experiments relative to these insects, I filled the cells of the batteries employed with nothing but common water. The lower shelf merely supported a wide-mouthed bottle, to receive the drops as they fell from the second funnel. When the basin was nearly emptied, the fluid was poured back again from the bottle below into the basin above, without disturbing the position of the stone. It was by mere chance that I selected this volcanic substance, choosing it from its partial porosity; nor do I believe that it had the slightest effect in the production of the insects to be described. The fluid with which I filled the basin was made as follows.

I reduced a piece of black flint to powder, having first exposed it to a red heat and quenched it in water to make it friable. Of this powder I took two ounces, and mixed them intensely with six ounces of carbonate of potassa, exposed them to a strong heat for fifteen minutes in a black lead crucible in an air furnace, and then poured the fused compound on an iron plate, reduced it to powder while still warm, poured boiling water on it, and kept it boiling for some minutes in a sand bath. The greater part of the soluble glass thus fused, was taken up by the water, together with a portion of alumina from the crucible. I should have used one of silver, but had none sufficiently large. To a portion of the silicate of potassa thus fused, I added some boiling water to dilute it, and then slowly added hydrochloric acid to supersaturation. A strange remark was made on this part of the experiment at the meeting of the British Association at Liverpool, it being then gravely stated, that it was impossible to add an acid to a silicate of potassa without precipitating the silica! This, of course, must be the case, unless the solution be diluted with water. My object in subjecting this fluid to a long-continued electric action through the intervention of a porous stone, was to form, if possible, crystals of silica at one of the poles of the battery, but I failed in accomplishing this by those means. On the fourteenth* day from the commencement of the experiment, I observed, through a lens, a few small whitish excrescences or nipples projecting from about the middle of the electrified stone, and nearly under the dropping of the fluid above. On the eighteenth* day these projections enlarged, and seven or eight filaments, each of them longer than the excrescence from which it grew, made their appearance on each of the nipples. On the twenty-second* day these appearances were more elevated and distinct, and on the twenty-sixth* day each figure assumed the form of a perfect insect, standing erect on a few bristles which formed its tail. Till this period I had no notion that these appearances were any other than an incipient mineral formation; but it was not until the twenty-eighth day, when I plainly perceived these little creatures move their legs that I felt any surprise, and I must own that when this took place, I was not a little astonished. I endeavoured to detach with the point of a needle, one or two of them from its position on the stone, but they immediately died, and I was obliged to wait patiently for a few days longer, when they separated themselves from the stone, and moved about at pleasure, although they had been for some time after their birth apparently averse to motion. In the course of a few weeks about a hundred of them made their appearance on the stone. I observed that at first each of them fixed itself for a considerable time in one spot, appearing, as far as I could judge, to feed by suction; but when a ray of light from the sun was directed upon it, it seemed disturbed, and removed itself to the shaded part of the stone. Out of about a hundred insects, not above five or six were born on the south side of the stone. I examined some of them with the microscope, and observed that the smaller ones appeared to have only six legs, but the larger ones eight. It would be superfluous to attempt a description of these little mites when so excellent a one has been transmitted from Paris.† It seems that they are of the genus *Acarus*, but of a species not hitherto observed. I have had three separate formations of similar insects at different times, from fresh portions of the same fluid, with the same apparatus. As I considered the result of this experiment rather extraordinary, I made some of my friends acquainted with it, amongst whom were some highly scientific gentlemen, and they plainly perceived the insect in various states. I likewise transmitted some of them to one of our most distinguished physiologists in London, and the opinion of this gentleman, as well as of other eminent persons to whom he showed them, coincided with that of the gentlemen of the Academie des Sciences, as to their genus and species. *I have never ventured an opinion as to the cause of their birth,* and for a very good reason—I was unable to form one. The most simple solution of the problem which occurred to me, was, that they arose from ova deposited by insects floating in the atmosphere, and that they might possibly be hatched by the electric action. Still I could not imagine that an ovum could shoot out filaments, and that those filaments would become bristles; and moreover, I could not detect, on the closest examination, any remains of a shell. Again, we have no right to assume that electric action is necessary to vitality, until such fact shall have been most distinctly proved. I next imagined, as others have done, that they might have originated from the water, and consequently made a close examination of several hundred vessels, filled with the same water as that which held in solution the silicate of potassa, in the same room, which vessels constituted the cells of a large Voltaic battery, used without acid. In none of these vessels could I perceive the trace of an insect of that description. I likewise closely examined the crevices and most dusty parts of the room with no better success. In the course of some months, indeed, these insects so increased, that when they were strong enough to leave their moistened birth-place, they issued out in different directions, I suppose, in quest of food; but they generally huddled together under a card or piece of paper in their neighbourhood, as if to avoid light and disturbance. In the course of my experiments upon other matters, I filled a glass basin with a concentrated solution of silicate of potassa without acid, in the middle of which I placed a piece of brick, used in this neighbourhood for domestic purposes, and consisting mostly of silica. Two wires of platina connected either end of the brick with the poles of a Voltaic battery of sixty-three pairs of plates, each about two inches square. After many months' action, silica in a gelatinous state formed in some quantity round the bottom of the brick, and as the solution evaporated, I replaced it by fresh additions, so that the outside of the glass basin being constantly wet by repeated overflowings, was, of course, constantly electrified. On this outside, as well as on the edge of the fluid within, I one day perceived the well-known whitish excrescence with its projecting filaments. In the course of time they increased in number, and as they successively burst into life, the whole table on which the apparatus stood, at last was covered with similar insects, which hid themselves wherever they could find a shelter. Some of them were of different sizes, there being a considerable difference in this respect between the larger and smaller; and they were plainly perceptible to the naked eye as they nimbly crawled from one spot to another. I closely examined the table with a lens, but could perceive no such excrescence as that which marks their incipient state, on any part of it. While these effects were taking place in my electrical room, similar formations were making their appearance in another room, distant from the former. I had here placed on a table, three Voltaic batteries unconnected with each other. The first consisted of twenty pairs of two-inch plates, between the poles of which I placed a glass cylinder filled with a concentrated solution of silicate of potassa, in which was suspended a piece of clay slate by two platina wires connected with either pole of the battery. A piece of paper was placed on the top of the cylinder to keep out the dust. After many months' action, gelatinous silica in various forms was electrically attracted to the slate, which it coated in rather a singular manner, unnecessary here to describe. In the course of time I observed similar insects in their incipient state forming around the edge of the fluid within the jar, which, when perfect, crawled about the inner surface of the paper with great activity. The second battery consisted of twenty pairs of cylinders, each equal to a four-inch plate. Between the poles of this I interposed a series of seven glass cylinders, filled with the following concentrated solutions:—1. Nitrate of copper: 2. Sub-carbonate of potassa: 3. Sulphate of copper; 4. Green sulphate of iron: 5. Sulphate of zinc: 6. Water acidified with a minute portion of hydrochloric acid: 7. Water poured on powdered metallic arsenic, resting on a copper cup, connected with the positive pole of the battery. All these cylinders were electrically united together by arcs of sheet copper, so that the same electric current passed through the whole of them.

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* Plate VIII. denoted by the fig. *a*, *b*, *c*, and *d*.

† This description, with a magnified figure of the insect will appear in our next number. Edit.

After many months' action, and consequent formation of certain crystalline matters, which it is not my object here to notice, I observed similar excrescences with those before described at the edge of the fluid in every one of the cylinders, excepting the two which contained the carbonate of potassa, and the metallic arsenic; and in due time a host of insects made their appearance. It was curious to observe the crystallised nitrate and sulphate of copper, which formed by slow evaporation at the edge of the respective solutions, dotted here and there with these hairy excrescences. At the foot of each of the cylinders I had placed a paper ticket upon the table, and on lifting them up I found a little colony of insects under each, but no appearance whatever of their having been born under their respective papers, or on any part of the table. The third battery consisted of twenty pairs of cylinders, each equal to a three-inch plate. Between the poles of this I interposed likewise a series of six glass cylinders, filled with various solutions, in only one of which I obtained the insect. This contained a concentrated solution of silicate of potassa. A bent iron wire, one-fifth of an inch in diameter, in the form of an inverted syphon, was plunged some inches into this solution, and connected it with the positive pole, whilst a small coil of fine silver wire joined it with the negative.

After some months' electrical action, gelatinous silica enveloped both wires, but in much greater quantity at the positive pole; and in about eight months from the commencement of the experiment, on examining these two wires very minutely, by means of a lens, having removed them from the solution for that purpose, I plainly perceived one of these incipient insects upon the gelatinous silica on the silver wire, and about half an inch below the surface of the fluid, when replaced in its original position. In the course of time, more insects made their appearance, till, at last, I counted at once three on the negative and twelve on the positive wire. Some of them were formed on the naked part of the wires, that is, on that part which was partially bare of gelatinous silica: but they were mostly imbedded more or less in the silica, with eight or ten filaments projecting from each beyond the silica. It was perfectly impossible to mistake them, after having made oneself master of their different appearances; and an occasional motion in the filaments of those that had been the longest formed was very perceptible, and observed by many of my visitors, without my having previously noticed the fact to them. Most of these productions took place from half to three-quarters of an inch under the surface of the fluid, which, as it evaporated very slowly, I kept to the same level by adding fresh portions. As some of these insects were formed on the inverted part of the syphon-shaped wire, I cannot imagine how they contrived to arrive at the surface, and to extricate themselves from the fluid: yet this they did repeatedly; their old places were vacated, and others were born in new ones. Whether they were in an imperfect state (except just at the commencement of their formation), or in a perfect one, they had all the distinguishing characteristic of bristles projecting from their bodies, which occasioned the French *savans* to remark that they resembled a microscopic porcupine. I must not omit to state, that the room in which these three batteries were acting was kept almost constantly darkened. It was not my intention to make known these observations until I myself should be better informed about the matter. Chance led to the publication of an erroneous account of them, which I was under the necessity of explaining. It is so difficult to arrive at the truth, that mankind would do better to lend their assistance to explore what may be worth investigating, than to endeavour to crush in its bud that which might otherwise expand into a flower. In giving this account, I have merely stated those circumstances regarding the appearance of insects, which I have noticed during my investigations into the formation of mineral matters; I have never studied physiology, and am not aware under what circumstances the birth of this class of insects is usually developed. In my first experiment I had made use of flannel, wood, and a volcanic stone; in the last, none of these substances were present. I never, for a moment, entertained the idea that the electric fluid had animated the organic remains of insects, or fossil eggs, previously existing in the stone or the silica; and have formed no visionary theory which I would travel out of my way to support. I have since repeated these latter experiments in a third room, in which there are now two batteries at work. One consisting of eleven pairs of cylinders, made of four-inch plates between the poles of which is placed a glass cylinder, filled with silicate of potassa, in which is suspended a piece of slate between two wires of platina, as before, and covered loosely with paper. Here, again, is another crop of insects formed. The other battery consists of twenty pairs of cylinders, the electric current of which is passed through six different solutions in glass cylinders, in three of which only is the insect formed, viz., 1st. in nitrate of copper; 2d. in sulphate of copper, in each of which the insect is only produced at the edge of the fluid, as far as I can make out; and 3d. by the old apparatus of coiled silver and iron wire in silicate of potassa, as before. There are now forming on the bottom of this positively electrified wire similar insects, at the distance of fully two inches below the surface of the fluid. On examining these, I have lately noticed a peculiar quality they possess whilst in an incipient state. After being kept some minutes out of the solution, they contract their filaments, so as, in some cases, wholly, and in others partially, to disappear. I at first thought they were destroyed; but, on examining the same spots, on the next day, they were as perceptible as before. In this respect, they seem not unlike the zoophytes, which adhere to the rocks on the sea-shore and which contract on the approach of a finger. I may likewise remark, that I have not been able to detect their eyes, even when viewed under a powerful microscope, although I once fancied I perceived them. The extreme heat of summer and cold of winter do not appear favourable to their production, which succeeds best, I think, in spring and autumn. As in the above account I have occasionally made use of the word "formation," I beg that it might be understood that I do not mean *creation,* or any thing approaching to it. I am not aware that I have any thing more to add, except the few remarks I shall conclude with.

1st. I have not observed a formation of the insect, except on a moist and electrified surface, or under an electrified fluid. By this *I do not mean to assert that electricity has any thing to do with their birth,* as I have not made a sufficient number of experiments to prove or disprove it; and besides, I have not taken those necessary precautions which present themselves even to an unscientific view. These precautions are not so easy to observe as may at first sight appear. It is, however, my intention to repeat these experiments, by passing a stream of electricity through cylinders filled with various fluids under a glass receiver inverted over mercury, the greatest possible care being taken to shut out extraneous matter. Should there be those who blame me for not having done this before, to such I answer that, independent of a host of other hindrances, which it is not in my power to set aside, I have been closely pursuing a long train of experiments on the formation of crystalline matters by the electric agency, and now different modifications of the Voltaic battery; in which I am so interested, that none but the ardent can conceive what is not in my power to describe.

2dly. These insects do not appear to have originated from others similar to themselves, as they are formed in all cases with access of moisture, and in some cases two inches below the surface of the fluid in which they are born; and if a full grown and perfect insect be let fall into any fluid, it is infallibly drowned.

3dly. I believe they live for many weeks: occasionally I have found them dead in groups, apparently from want of food.

4thly. It has been frequently suggested to me to repeat these experiments without using the electric agency; but this would be by no means satisfactory, let the event be what it would. It is well known that saline matters are easily crystallized without subjecting them to the electric action; but it by no means follows that, because artificial electricity is not applied, such crystals are formed without the electric influence. I have made so many experiments on electrical crystallization, that I am firmly convinced in my own mind, that electric attraction is the cause of the formation of every crystal, whether artificial electricity be applied or not. I am, however, well aware of the difficulty of getting at the truth in these matters, and of separating cause from effect. It has often occurred to me how it is that such numbers of animalcules are produced in flour and water, in pepper and water? also, the insects which infest fruit trees after a blight? Does not a chemical change take place in the water, and likewise in the sap of the tree *previous* to the appearance of these insects, and is or is not every chemical change produced by electric agency? In making these observations I seek to mislead no one. The book of nature is opened wide to our view by the Almighty power, and we must endeavour, as far as our feeble faculties will permit, to make a good use of it; always remembering, that however the timid may shrink from investigation, the more completely the secrets of nature are laid bare, the more effectually will the power of that Great Being be manifested, who seems to have ordained, that

"Order is Heaven's first law."

I beg to remain, in the mean time, my dear Sir,

Your's, very sincerely,
ANDREW CROSSE.

Broomfield,
December 27, 1837.

P.S. Since writing the above account, I have obtained the insects on a bare platina wire plunged into fluo-silicic acid, *one inch below* the surface of the fluid at the negative pole of a small battery of two-inch plates in cells filled with water. This is a somewhat singular fluid for these insects to breed in, who seem to have a flinty taste, although they are by no means confined to silicious fluids. This fluo-silicic acid was procured from London some time since, and consequently made of London water; so that the idea of their being natives of the Broomfield water is quite set aside by this result. The apparatus was arranged as follows; Fig. 58, Plate VIII., a glass basin (a pint one) partly filled with fluo-silicic acid to the level 1. 2, a small porous pan, made of the same materials as a garden pot, partly filled with the same acid to the level 2, with an earthen cover, 3, placed upon it, to keep out the light, dust, &c. 4, a platina wire connected with the positive pole of the battery, with the other end plunged into the acid in the pan, and twisted round a piece of common quartz; on which quartz, after many months' action, are forming singularly beautiful and perfectly formed crystals of a transparent substance, not yet analyzed, as they are still growing. These crystals are of the modification of the cube, and are of twelve or fourteen sides. The platina wire passes under the cover of the pan. 5, a platina wire connected with the negative pole of the same battery, with the other end dipping into the basin, an inch or two below the fluid; and, as well as the other, twisted round a piece of quartz. By this arrangement it is evident that the electric fluid enters the porous pan by the wire 4, percolates the pan, and passes out by the wire 5. It is now upwards of six or eight months (I cannot at this moment put my hand on the memorandum of the date) since this apparatus has been in action, and though I have occasionally lifted out the wires to examine them by a lens, yet it was not till the other day that I perceived any insect, and there are now three of the same insects, in their incipient state, appearing on the naked platina wire at the bottom of the quartz *in the glass basin at the negative pole.* These insects are very perceptible and may be represented thus (magnified): fig. 59, 1 the platina wire, 2 the quartz, 3 the incipient insects. It should be observed that the glass basin, fig. 58, has always been loosely covered with paper. The incipient appearance of the insect has already been described. The filaments which project are in course of time seen to move, before the perfect insect detaches itself from its birth-place.

(Plate VIII, fig. 56,) front view of the filtering apparatus, by the use of which, the insect described made its first appearance. (A. B.) two of the four uprights or legs issuing from the base (C), supporting a moveable shelf (D); which shelf is kept in its place by four pins (E) passing through the four uprights, and may be raised or lowered at pleasure. (F) the top shelf which has an aperture cut in it to receive the Wedgewood ware funnel (G). (H) a quart basin standing on an unseen support within the funnel (G), which support is a circular piece of wood with holes cut in it to allow the free passage of the fluid between the basin and funnel. This basin is filled with the fluid required, which is conveyed out of it by the strip of flannel (I), hanging over the outside of the basin, and inside the funnel, and which, consequently, falls in successive drops through the funnel (G) upon the stone (K), which is supported by the glass funnel (L), kept constantly electrified by the two platina wires (M N), resting on the opposite sides of it, and connected with the opposite poles of a voltaic battery. (O) a wide mouthed bottle standing on the base (C), to receive the fluid as it falls from the second funnel (L). From this bottle, when required, it is poured back again into the basin (H) without disturbing the stone (K).

(Fig. 56, A) a glass cylindrical vessel containing about a quarter of a pint filled with a concentrated solution of silicate of potash. (B) a fine silver wire formed into a coil, which is immersed into the fluid in the cylinder the other end being connected with the negative pole of the battery. (C) an iron wire about 1-fifth of an inch in diameter, bent somewhat in the form of an inverted syphon, immersed in the same vessel, and connected with the positive pole of the battery. (D D) insects in their incipient state making their appearance, some on the gelatinous silica, which partially covers the wire, and some on the naked wire itself. These insects appear magnified.

XLIII. *Notice of the Electro-Magnetic Machine of* Mr. Thomas Davenport, *of Bandon, near Rutland, Vermont, U. S.*[*]

Many years have passed since motion was first produced by galvanic power. The dry columns of De Luc and Zamboni, caused the vibration of delicate pendulums and the ringing of small bells, for long periods of time, even several years without intermission.

In 1819—20, Professor Œrsted, of Copenhagen, discovered that magnetism was evolved between the poles of a galvanic battery. Professor Sweigger, of Halle, Germany, by his galvanic multiplier, succeeded in rendering the power manifest when the galvanic battery was nothing more than two small wires, one of copper and the other of zinc, immersed in as much acidulated water as was contained in a wine glass. The power thus evolved was made to pass through many convolutions of insulated wire, and was thus augmented so as to deflect the magnetic needle sometimes even 90°. Professor Moll, of Utrecht, by winding insulated wire round soft iron, imparted to it prodigious magnetic power, so that a horse-shoe bar, thus provided, and connected with a galvanic battery, would lift over one hundred pounds. About the same time, Mr. Joseph Henry, of Albany, now Professor Henry, of Princeton College, by a new method of winding the wire, obtained an almost incredible magnetic force, lifting six or seven hundred pounds, with a pint or two of liquid, and a battery of corresponding size: nor did he desist until a short time after, he lifted thousands of pounds, by a battery of larger size, but still very small, (1830).

This gentleman was not slow to apply his skill to the generation of motion, and a successful attempt of his is on record in this journal (Silliman's, vol. xx. p. 340). A power was thus applied to the movement of a machine by a beam suspended in the centre, which performed regular vibrations in the manner of a beam of a steam engine. This is the original application from which have sprung, or at least, to which have succeeded, several similar attempts, both in this country and in Europe. A galvanic machine was reported to the British Association in 1835, by Mr. Mc Gauly, of Ireland, and he has renewed his statements of successful experiments at the late meeting at Bristol. Mr. Sturgeon, of Woolwich, England, also reports a galvanic machine as being in use on his premises for pumping water, and for other mechanical purposes.*

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[*] From Silliman's Journal for April, 1837.

But I believe that Mr. Davenport, named at the head of this notice, has been more successful than any other person in this discovery† of a galvanic machine of great simplicity and efficiency. During the last two or three years, much has been said of this discovery in the newspapers, and it is probable that in a future number of this journal, (Silliman's) drawings and an accurate description of the machine may be given. Having been recently invited to examine a working model, in two varieties of form, and to report the result, I shall now attempt nothing more than a general description, such as may render intelligible the account I am to give.

1. *The Rotatory Machine, composed of revolving electro-magnets, with fixed permanent magnets.*

This machine was brought to Newhaven, March 16, 1837, by Mr. Israel Slade, of Troy, N. Y., and by him set in motion for my examination. The moving part is composed of two iron bars placed horizontally, and crossing each other at right angles. They are both five and a half inches long, and they are terminated at each end by a segment of a circle made of soft iron; these segments are each three inches long in the chord line, and their position, as they are suspended upon the ends of the iron bars, is horizontal.

This iron cross is sustained by a vertical axis standing with its pivot in a socket, and admitting of easy rotation. The iron cross bars are wound with copper wire, covered by cotton, and they are made to form, at pleasure, a proper connexion with a small circular battery, made of concentric cylinders of copper and zinc, which can be immersed in a quart of acid;

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* Sturgeon's Annals of Electricity, Magnetism, &c., No. 1, vol. 1, October, 1836.

† Mr. Davenport appears to have been strictly the inventor of a method of applying galvanism to produce rotary motion.

ulated water. Two semicircles of strongly magnetized steel, form an entire circle, interrupted only at the two opposite poles, and within this circle, which lies horizontally, the galvanized iron cross moves, in such manner that its iron segments revolve parallel, and very near to the magnetic circle, and in the same plane. Its axis at its upper end, is fitted by a horizontal cogg wheel, to another and larger vertical wheel, to whose horizontal axis, a weight is attached and raised by the winding of a rope. As soon as the small battery destined to generate the power, is properly connected with the machine, and duly excited by diluted acid, the motion begins, by the horizontal movement of the iron cross, with its circular segments or flanges. By the galvanic connexion, these crosses and their connected segments, are magnetized, acquiring north and south polarity at their opposite ends, and being thus subjected to the attracting and repelling force of the circular fixed magnets, a rapid horizontal movement is produced, at the rate of two hundred to three hundred revolutions in a minute, when the small battery was used, and over six hundred with a calorimotor of a large size. The rope was wound up with a weight of fourteen pounds attached, and twenty-eight pounds were lifted from the floor. The movement is instantly stopped by breaking the connexion with the battery, and then reversed by simply interchanging the connexion of the wires of the battery with those of the machine, when it becomes equally rapid in the opposite direction.

The machine, as a philosophical instrument, operates with beautiful and surprising effect, and no reason can be discovered why the motion may not be indefinitely continued. It is easy to cause a very gradual flow of the impaired or exhausted acid liquor from, and of fresh acidulated water into, the recepticle of the battery, and whenever the metal of the latter is too much corroded to be any longer efficient, another battery may be instantly substituted, and that even before the connexions with the old battery are broken. As to the energy of the power, it becomes at once a most interesting enquiry, whether it admits of indefinite increase? To this enquiry it may be replied, that provided the magnetism of both the revolving cross and of the fixed circle can be indefinitely increased, then no reason appears why the energy of the power cannot also be indefinitely increased. Now, as magnets of the common kind, usually called permanent magnets, find their limits within, at most, the power of lifting a few hundred pounds, it is obvious that the revolving galvanic magnet must, in its efficiency, be limited, by its relation to the fixed magnet. But it is an important fact, discovered by experience, that the latter is soon impaired in its power by the influence of the revolving galvanic magnet, which is easily made to surpass it in energy, and thus, as it were, to overpower it. It is obvious, therefore, that the fixed magnet, as well as the revolving one, ought to be magnetized by galvanism, and then there is every reason to believe that the relative equality of the two, and of course their relative energy, may be permanently supported, and even carried to an extent much greater than has hitherto been attained.

2. *Rotating Machine, composed entirely of electro-magnets, both in its fixed and revolving members.*

A machine of this construction has been, this day, March 29, 1837, exhibited to me by Mr. Thomas Davenport himself, who came from New York to New Haven for that purpose.

It is the same machine that has already been described, except that the exterior fixed circle is now composed entirely of electro-magnets.

The entire apparatus is therefore constructed of soft magnetic iron, which being properly wound with insulated copper wire, is magnetized in an instant, by the power of a very small battery.

The machine is indeed the identical one used before, except that the exterior circle of permanent magnets is removed, and in its place is arranged a circle of soft iron, divided into two portions to form the poles.

These semicircles are made of hoop iron, one inch in width and one-eighth of an inch in thickness. They are wound with copper wire, insulated with cotton—covering about ten inches in length on each semicircle and returning upon itself, by a double winding, so as to form two layers of wire, making on both semicircles, about one thousand and five hundred inches.

The iron was not wound over the entire length of the steel* semicircles; but both ends were left projecting, and being turned inwards, were made to conform to the bend of the other part, as in fig. , Plate VIII, which is intended to represent one of them; each end that is turned inward, and not wound, is about one-third of the length of the semicircle. These semicircles being thus fitted up, so as to become, at pleasure, galvanic magnets, were placed in the same machine as has been already described, and occupied the same place that the steel magnets did before. The conducting wires were so arranged, that the same current that charged the magnets of the motive wheel, charged the stationary ones, placed around it, only one battery being used. It should be observed, that the stationary galvanic magnets thus substituted for the permanent steel ones, were only about half the weight of the steel magnets. This modification of the galvanic magnet, is not, of course, the best form for efficiency; this was used merely to try the principle, and this construction may be superseded by a different and more efficient one. But with this arrangement, and notwithstanding the imperfection of the mechanism of the machine—when the battery, requiring about one quart of diluted acid to immerse it, was attached, it lifted sixteen pounds very rapidly, and when the weight was removed, it performed more than six hundred revolutions per minute.

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* The word "steel" is obviously a mistake, as we are expressly informed in the preceding paragraph, that "hoop iron" was employed. EDIT.

So sensible was the machine to the magnetic power, that the immersion of the battery one inch into the acidulated water, was sufficient to give it rapid motion, which attained its maximum, when the battery was entirely immersed. It appeared to me that the machine had more energy with the electro-magnets, than with those that were permanent, for with the small battery, whose diameter was three inches and a half—its height five inches and a half, and the number of concentric cylinders three of copper and three of zinc, the instrument manifested as great power as it had done with the largest batteries, and even with a large calorimotor, when it was used with a permanent instead of a galvanic magnet. With the small battery and with none but electro or galvanic magnets, it revolved, with so much energy as to produce a brisk breeze, and powerfully to shake a large table on which the apparatus stood.

Although the magnetization of both the stationary and revolving magnets was imparted by one and the same battery, the magnetic power was not immediately destroyed by breaking the connexion between the battery and the stationary magnet; for, when this was done, the machine still performed its revolutions with great, although diminished energy; in practice this might be important, as it would give time to make changes in the apparatus, without stopping the movement of the machine.

It has been stated by Dr. Ritchie, in a late number of the London and Edinburgh Phil. Magazine, that electro-magnets do not attract at so great a distance as permanent ones, and therefore are not well adapted for producing motion. On this point, Mr. Davenport made the following experiment, of which I was not a witness, but to which I give full credit, as it was reported to me by Mr. Slade, in a letter dated New York, March 24, 1837.

Mr. Davenport suspended a piece of soft iron with a long piece of twine, and brought one pole of a highly charged steel magnet within the attracting distance, that is, the distance at which the iron was attracted by the magnet; by measurement, it was found that the steel magnet attracted the iron one inch and one fourth. A galvanic magnet was next used of the same lifting power, and consequently of much less weight; the attracting distance of this magnet was found to be one inch and three-fourths, showing a material gain in favour of the galvanic magnet. Mr. Slade enquires, "has Mr. Ritchie's magnet been so constructed as to give a favourable trial of this principle?*" Mr. Davenport informs me that each increase in the number of wires has been attended with an increase of power.

Conclusions.

1. It appears then, from the facts stated above, that electro-magnetism is quite adequate to the generation of rotating motion.

2. That it is not necessary to employ permanent magnets in any part of the construction, and that electro-magnets are far preferable, not only for the moving but for the stationary parts of the machine.

3. That the power generated by electro-magnetism may be indefinitely prolonged, since, for exhausted acids and corroded metals, fresh acids and batteries, kept always in readiness, may be substituted, even without stopping the movement.

4. That the power may be increased beyond any limit hitherto attained, and probably beyond any which can be *with certainty* assigned;—since, by increasing all the members of the apparatus, due reference being had to the relative proportionate weight, size, and form of the fixed and moveable parts,—to the length of the insulated wires and the manner of winding them;—and to the proper size and construction of the battery, as well as to the nature and strength of acid or other exciting agent, and the manner of connecting the battery with the machine, it would appear certain, that the power must be increased in some ratio which experience must ascertain.

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* The writer says, "This question I am not able to answer, as I have not seen any account of the apparatus or of the experiment, but only the result.

5. As electro-magnetism has been experimentally proved to be sufficient to raise and sustain several thousands of pounds, no reason can be discovered why, when the acting surfaces are, by skilful mechanism, brought as near as possible, without contact, the continued exertion of the power should not generate a continued rotatory movement, of a degree of energy inferior indeed to that exerted in actual contact, but still nearly approximating it.

6. As the power can be generated cheaply and certainly,—as it can be continued indefinitely—as it has been very greatly increased by very simple means—as we have no knowledge of its limit, and may therefore presume on an indefinite augmentation of its energy, it is much to be desired, that the investigations should be prosecuted with zeal, *aided by correct scientific knowledge*, by *mechanical skill*, and by *ample funds*. It may, therefore, be reasonably hoped, that science and art, the handmaids of discovery, will both receive from this interesting research, a liberal reward.

Science has thus, most unexpectedly, placed in our hands a new power of great but unknown energy.

It does not wake the winds from their caverns; nor give wings to water by the urgency of heat; nor drive to exhaustion the muscular power of animals; nor operate by complicated mechanism; nor accumulate hydraulic force by damming the vexed torrents; nor summon any other form of gravitating force, but, by the simplest means—the mere contact of metallic surfaces of small extent, with feeble chemical agents, a power everywhere diffused through nature, but generally concealed from our senses, is mysteriously evolved, and by circulation in insulated wires, it is still more mysteriously augmented, a thousand and a thousand fold, until it breaks forth with incredible energy; there is no appreciable interval between its first evolution and its full maturity, and the infant starts up a giant.

Nothing since the discovery of gravitation and of the structure of the celestial systems, is so wonderful as the power evolved by galvanism, whether we contemplate it in the muscular convulsions of animals, the chemical decompositions, the solar brightness of galvanic light, the dissipating consuming heat, and, more than all, in the magnetic energy, which leaves far behind all previous artificial accumulations of this power, and reveals, as there is full reason to believe, the grand secret of terrestrial magnetism itself.

New Haven, B. S.

March 31, 1837.

Claim of Thomas Davenport.

In the words of the patent, taken out, this invention "consists in applying magnetic and electro-magnetic power as a moving principle for machinery, in the manner described, or in any other substantially the same principle. (Is it possible!!!)

"Mr. Davenport first saw a galvanic magnet in December, 1833, and from the wonderful effects produced, by suspending a magnet of one hundred and fifty pounds from a small galvanic battery, he immediately inferred, without any knowledge of the theory or the experiments of others, that he could propel machinery by galvanic magnetism. He purchased the magnet and produced his first rotatory motion in July, 1834. In July, 1835, he submitted his machine to Professor Henry, of Princeton, New Jersey, also without any knowledge of Professor Henry's experiments in producing a vibratory motion. From this gentleman he received a certificate testifying to the originality and importance of the invention."

Mr. Davenport is by occupation a blacksmith, with only a common education, but with uncommon intelligence; his age about thirty-five. Mr. Ransom Cook, of Saratoga Springs, is associated with Mr. Davenport, and has rendered essential service by the improvements he has made in the machine, and by his assistance in bringing the subject before the public in the most effectual way. Arrangements have been made to take out the patent in Europe.

P. S. The proprietors are constructing a machine of seven inches in diameter, and also one of two feet in diameter. Galvanic magnets will be used as the moving and stationary magnets of each.

XLIV. *On an instrument proposed for measuring the expansion of Solid Bodies, and which may also be used as a Thermometer.* By W. W. Mather, A. M., and Lieut. U. S. Army.*

It has long been a desideratum to measure the expansion of bodies, and the changes of temperature, more accurately than these can be done by the instruments which are or have been in use. Every scientific man is aware of the practical utility of a solution of the two points mentioned, and more particularly, in geodesic operations, where the accuracy of extensive surveys is dependent on a rigid determination of the length of the base line, and in the true determination and verification of weights, and of measures of length and capacity. An instrument for such purposes, becomes more valuable in proportion to its accuracy and its capacity, for rigid verification. The mode of determining the expansion of solids employed by Messrs. Lavoiser and Laplace, is of all the methods that have been employed, the least exceptionable, and probably their determinations, as far as they experimented, are close approximations to the true expansions.*

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* From Silliman's Journal.

Mr. Hassler's method of measuring the expansion of his measuring rods by means of micrometer screws, is very ingenious, and it is surprising how close his approximations are to those of Lavoiser and Laplace, when we consider the possible error arising from the probable slight flexture in his long wire rods.†

Thermometers all labor under an objection, which it has not, hitherto, been practicable altogether to obviate, and the one that I shall propose, will probably labor in a slight degree, under the same objection, if employed at a higher temperature than the ordinary ranges of atmospheric heat and cold. This objection, in the thermometers hitherto used, arises from the different rates of expansion of the bodies used in their construction, and from there being no means of testing rigidly, the rates of expansion in each individual case. The fact is notorious, that scarcely any two thermometers, however carefully constructed, are strictly comparable, and hence, the utility of the instrument in minute scientific investigations is much less than might be expected. To the same defect are to be attributed, in part, the different results of philosophers in some of their investigations on temperature, and as an example of this, the temperature of distilled water at its maximum density may be quoted.

Although the flint glass experimented on by Biot and Arago, had such a rate of expansion as to counteract almost exactly the increasing rate of expansion in mercury and thus produce the effect of a uniform expansion of the mercury, yet the flint glass manufactured for thermometer tubes is not composed of the same proportions of the materials in different manufactories and is of different densities and rates of expansion, and hence it follows, that its rate of expansion in most cases differs from that of mercury, and, consequently, the compensation will not be exact.

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* A description of their instrument and mode of experimenting may be seen in Biot, Traite de Physique, or more in detail in the Memoirs de l' Institute.

† Vide Hassler on the Coast Survey of the U. S. in the Am. Phil. Transactions.

Again, glass is highly elastic, and the increasing length of the column of mercury, causes the bulb, which is of thin glass, to increase its capacity by the effect of hydrostatic pressure, and as all the bulbs have not the same thickness in relation to their capacities, it follows, that in different thermometers with even the same length of columns of mercury, the effect of the hydrostatic pressure, in increasing the capacities of the bulbs, would prevent them from being strictly comparable. There are other sources of error too well known to require notice.

Of all the metallic thermometers, those of Borda and Breguet, are perhaps least objectionable. The first, to be very accurate, requires the bars to be very long and it then becomes cumbrous; the other has too many sources of error to be regarded as an accurate instrument.

Air thermometers are the most accurate of all, yet they labor under this objection, that while the air expands uniformly for equal increments of temperature, the containing body expands in a slightly increasing ratio as the temperature increases, thus enlarging its capacity, so that a less temperature is indicated than the true one. This is true as well with the differential as with the other air thermometers, but the inaccuracy is so slight as to render it inappreciable at ordinary ranges of temperature.

The published details of the measurement of the base lines of various extensive trigonometrical surveys, of the measurement of the arcs of the meridian, and of the establishing of standards of weight, length, and capacity, show how much labor, thought, and science, have been employed in endeavoring to arrive at a rigid accuracy. The thermometer has been the principal stumbling block, in consequence of no means having been contrived to insure uniformity in the indications of this instrument, and there being no means of testing its accuracy to the extent that many scientific researches demand; and as corrections for changes of temperature enter into all the investigations mentioned above, uniformity in the results can be obtained only when temperatures, and the changes of volume resulting from varied temperatures can be accurately measured.

In all accurate measurements of linear expansion in solids, the first object is, to have two points which shall remain invariably equidistant at all temperatures within the range of experiment; and the second, is, to provide the means of accurately measuring the variations in the length of the body under examination, when it is placed between these fixed points.

I propose to accomplish the first of these objects, by making use of *two bars of different metals, whose lengths are inversely proportional to their expansibilities*, on the principle of the compensation pendulum; that is, if both the bars be equally heated, the shorter bar shall expand exactly as much in length as the longer, and the distance $a\,b$, fig. 60, Plate VIII., shall be equal, at one temperature, to $a\,b$ at another.

To verify the accuracy of this equal expansion, I propose to use the combined bars fastened together at c, as a balance beam; the masses being so adjusted, as to throw the centre of gravity in the vertical plane passing through a, and perpendicular to the axis of the beam. A delicate knife edge is attached to the end of the short bar at a, like that of a balance, and it rests on polished cylinders of glass, traversely to their axes. The friction is thus rendered null, by the contact of the knife edge and the supporting cylinders being reduced almost to a mathematical point. After the centre of gravity shall have been brought indefinitely near to the knife edges, and below them, by the ordinary adjustments of a balance, so as to render it as delicate a balance as the inertia of the mass will allow, the combined mass is ready for experiments to test the equality of expansion. If it be now subjected to varied temperatures, (being exposed to each a sufficient time for its mass to be uniformly heated,) and it remains in equilibrio, the compensation is exact, and the expansion of each bar is equal in length.

This method, although sufficiently accurate for most purposes, has its limit in delicacy, and is not as exact as many philosophical investigations require. I propose to employ another and a better method of verifying the accuracy of the compensating expansibilities, one that is capable of demonstrating to any degree of exactness that may be desired, whether the expansion of one of the bars compensates perfectly for the expansion of the other; or in other words, whether the distance between the extremities of the two bars be uniformly the same at all temperatures to which the mass may be subjected.

This mode is, to use the combined mass as a pendulum, the knife edge of suspension being on the plane of the end of the longer bar, and the mass being so adjusted as to throw its centre of gravity in the plane of the extremity of the shorter bar. It is a well known mechanical principle that the product of the distance from the point of suspension to the centre of gravity, into the distance from the centre of gravity to the centre of oscillation is a constant quantity. From this it follows, that a pendulum keeping uniform time, must have its centre of oscillation to remain at a constant distance, else, the length of the pendulum varying, the time kept by it will vary. If the pendulum under consideration keeps uniform time at varied temperatures, it demonstrates that the centres of oscillation and gravity remain at invariable distances from the point of suspension, and, consequently, that the distance from the extremity of the longer to that of the shorter bar remains of a constant length and solves the proposition, viz. to obtain two points which shall remain at an invariable distance at different temperatures. Suppose the pendulum, at a mean temperature of 32° Farenheit, beats *m* times in a week, and at a mean temperature of 100° Farenheit it also beats *m* times in the same period; it follows that the distance of the centres of oscillation and gravity remain at a constant distance from the point of suspension, and that the compensation for unequal expansibility is exact. This mode of verification may be carried to any degree of exactness that circumstances may render expedient. One thing has been supposed that is not rigidly true, viz. that the metals used for the bars each expand uniformly for equal increments of temperature; but as the uses for which the instruments will be most valuable, do not require higher ranges of temperature than those of the atmosphere, this source of inaccuracy, if metals of high fusing points be selected, becomes almost infinitely small, and would scarcely be appreciated, even by the rigid verification above proposed.

The second point is, to provide the means of measuring accurately the variations in length of bodies placed for experiment between the fixed points.

This may be done by means of a micrometer screw with a graduated head and vernier attached; or in a better way by the bar of experiment C, fig. 61, which abuts firmly against the more exhaustible bar of the instrument A acting at its free end against the screw *d*. This screw is made with a very oblique thread so that it will thrust out with a small force and turn at the same time on its axis. By means of the rotation of this screw, the expansion of the bar C between the two invariable points M N may be measured.

The rotation of the screw *d* may be measured by a system of wheel-work like that of a watch, or, (as this is liable to some inaccuracies from complication) by means of a telescope mounted on the axis of the screw and perpendicular to it, and ranging over a graduated arc with a vernier, at a convenient distance. In this way a minute of a degree equal to $\frac{1}{21600}$ of a rotation, and perhaps a second of a degree equal to $\frac{1}{1296000}$ of a rotation of the screw might be measured. The limits of the delicacy of this determination will depend on the smallness of the screw which must be strong enough for the purpose named, and on the distance of the arc at which minute divisions can best be seen by the telescope. This method seems to be susceptible of much greater accuracy and more rigid verification than any one hitherto employed. In the practical use of the instrument there are only two corrections to be made and they are of the nature of constants which are to be applied in all the experiments except when the experimental bar is of the same metal as the screw. They are, 1st. the expansion of the screw *d* from zero, and 2d, the correction for the expansion of a steel plate of known thickness between the screw and the end of the bar C. This steel plate is acted against by the point of the screw, because this point would otherwise indent the bar and the screw be thrust out a less distance than the actual expansion.

The thermometer proposed is the same instrument as that which has been described, except that the same experimental bar C is always used, and that a spring is connected with the oblique threaded screw *d* so as to cause the point of the screw to be always pressing upon the end of the bar C. The temperature may be indicated by means of a graduated plate connected with the screw with a vernier attached analogous to those of a theodolite, by means of a system of wheels like a watch, or by means of a telescope mounted as before described. This thermometer labors under one objection, viz. that the different metals do not expand with perfect uniformity as their temperatures increase, but as in this instrument no temperatures much beyond the limits of atmospheric variation are proposed to be measured, this objection vanishes, for the metals of difficult fusibility are said to expand infinitely near to uniformity by equal increments of temperature within the range of atmospheric changes.

Another application of the principles of the same instrument is proposed for use, in connection with scales of equal parts, for making mathematical drawings. Every one who has attempted to make very accurate mathematical drawings, must be aware that a distance of any number of divisions of equal parts laid off at one temperature, differs from that laid off from the same number at another. It follows, that in accurately plotting the triangulations of extensive surveys, a practical difficulty would be experienced, and it is often experienced, when the plotting is continued through various atmospheric changes. The method proposed for obviating this difficulty, is, to have several of the pairs of bars described as maintaining a uniform distance between two points at different temperatures, and let the distance M N, in them respectively, be one inch, one foot, one yard, one metre, or any other convenient units. By means of these units, the common brass scale of equal parts usually seen in boxes of French mathematical instruments, *may be set* each day when commencing to draw. If the temperature of the room remain uniform during the day, and the scale be not handled to increase its temperature, the difficulty arising from expansion so far as it depends on the uniformity of the scales for measurement is obviated.

Those gentlemen engaged in plotting the triangulations of the coast survey of the United States, under the direction of Mr. Hassler, frequently experience the inconvenience arising from the expansion of their scales and paper. I trust that the above suggestion may tend to remove one of the many difficulties which they have to encounter.

Being under the impression that the instrument which I have proposed will obviate some of the difficulties in the measurements of dilitation and temperature, I may be excused for adding another to the long catalogue of those which have been heretofore described.

XLV. *Observations on the Variation of the Magnetic Needle, made at Yale College, in* 1834 *and* 1835; *by* Elias Loomis.*

About the middle of October, 1834, I commenced a series of observations on the diurnal variation of the magnetic needle. The instrument employed was a Variation Transit, by Dolland, belonging to the College. The needle is 5·4 inches in length, and the compass circle is graduated to quarter degrees. The azimuth circle is graduated to half degrees, and has three verniers, each reading to single minutes. The instrument was placed by a north window in North College upon a solid block of wood, resting on the floor, and so secured as to be free from all motion, except the unavoidable agitation of the building. There was no fire in the apartment where the instrument was placed, although its temperature was somewhat affected by a fire in the adjoining room. Before commencing the observations, all moveable iron was removed from the vicinity of the needle; and no change was made in this respect during the continuance of the observations. The several adjustments were carefully attended to. The levels were first corrected, so that the instrument might be turned quite round in azimuth, without sensibly moving the bubble in either level. I ascertained that the perpendicular wires of the transit, were truly perpendicular to the horizon, by pointing the instrument towards a star, and moving the telescope in altitude. I ascertained that the horizontal wire was truly horizontal, by causing a star to travel upon it, when the instrument was in the meridian. To ascertain if the line of collimation was perpendicular to the axis of rotation, I noted the instant of Polaris' passage at the first two wires; then reversed the axis and noted the third passage. The two intervals were very nearly equal. I ascertained that the line of collimation was in the same vertical plane with the meridian, as marked on the compass. This was done by pointing the telescope downwards towards the divisions on the compass, the focal distance being regulated by a small lens fitted to the object glass, and the central wire of the transit was made to coincide with the two zeros on the compass. Having made these preparatory adjustments, the instrument was finally placed upon the meridian, by noting the passage of Polaris. A meridian mark was fixed upon, at a considerable distance, and for verification, the passage of Polaris was repeatedly observed. As it required considerable time to turn the instrument in azimuth so as to read off by means of the verniers, these were seldom employed, but instead of them I used a compound microscope, and estimated the fraction of a quarter degree, (the smallest division on the compass,) by my eye. After some practice, I was able to do this with considerable accuracy, so that, as I judged, I was not liable to an error of more than one minute. As the first observations were necessarily imperfect, those which were made during the month of October, 1834, were rejected. A Fahrenheit's thermometer was placed about two feet above the transit, within the building, and was always observed at the same time with the needle. These observations were intended to be made at every hour of the day, from five or seven o'clock in the morning, till ten at night; yet some failures were absolutely unavoidable. At some hours of the day, the observations were continued uninterruptedly for a month together; while at a few hours, the observations were made only about half the time. This fact will explain some apparent anomalies in the following tables, particularly in the observations with the thermometer. During the period embraced in these observations, the needle has repeatedly suffered a sudden and irregular deflection to the amount, in three instances of more than a degree. A particular account of these irregularities will be given in the latter part of this article: and they have been excluded from the means in the following table, the object there being to exhibit the regular diurnal variation. To determine the influence of the building, I took the instrument out into the President's garden, where it was supposed the local attraction must be small, if any, and made repeated observations. The influence of the building was determined to be 1° 21′ 41″, which has accordingly been added to all the observations. The instrument was also carried out of the city, to a situation remote from any building, and nearly the same result obtained.

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* From Silliman's Journal.

Mean Monthly Declination of the Magnetic Needle at Yale College.

	5 A. M.	6 A. M.	7 A. M.	8 A. M.	9 A. M.	10 A. M.	11 A. M.	12 A. M.	1 P. M.	2 P. M.	3 P. M.	4 P. M.	5 P. M.	6 P. M.	7 P. M.	8 P. M.	9 P. M.	10 P. M.
1834.	′ ″	° ′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″
Nov.,		5 37 10	36 33	35 46	36 3	36 50	37 22	39 2	39 52	40 21	40 37	38 57	37 10	37 3	35 50	35 13	36 5	35 25
Dec.,		37 14	37 47	36 40	36 36	36 40	37 9	39 45	38 46	39 6	38 13	38 36	37 51	36 51	36 3	35 54	35 30	35 5
1835.																		
Jan.,		37 1	36 59	36 6	35 24	35 48	37 1	39 41	39 30	39 51	39 23	39 43	37 55	38 36	37 41	36 34	36 36	36 43
Feb.,		36 27	36 29	36 9	36 18	36 28	37 5	37 55	38 32	39 1	38 9	38 27	37 25	38 1	37 32	37 9	36 43	36 15
March,		36 57	36 29	34 49	34 53	35 43	36 29	37 33	38 56	39 48	40 14	38 59	37 56	38 28	37 49	37 38	36 44	36 38
April,		35 28	34 39	33 20	33 48	35 53	37 12	38 16	40 0	40 53	39 30	39 28	37 44	36 41	36 48	36 28	36 36	35 17
May,	40 11	39 1	36 57	37 14	37 52	40 11	44 7	48 43	49 7	48 49	48 41	46 9	43 0	42 38	40 11	41 46	41 8	41 33
June,	37 7	35 10	34 41	34 50	36 49	38 20	40 49	42 14	45 52	44 7	44 41	41 1	40 13	38 52	39 46	36 1	38 24	39 54
July,	35 41	33 50	34 41	33 0	34 45	36 57	38 32	39 5	42 3	41 52	43 3	39 14	38 44	38 15	37 29	37 47	37 35	36 4
Aug.,	35 14	32 36	31 45	34 8	34 54	37 9	42 21	42 19	45 15	44 57	42 41	44 18	41 34	39 26	37 41	38 46	37 45	35 34
Sept.,			37 18	38 3	39 48	45 21	47 48	51 11	51 21	50 26	49 41	46 18	45 41	48 1	45 41	46 41	43 1	41 41
Oct.,			43 46	40 18	41 31	43 23	46 1	50 2	50 32	50 54	50 57	49 58	48 38	49 1	48 11	49 30	46 6	49 56
Nov.,			48 0	46 11	47 36	48 0	50 46	52 38	52 48	53 50	52 59	52 47	49 32	51 6	49 21	47 17	46 11	49 18
Mean,			37 23	36 40	37 24	38 59	40 59	42 57	44 3	44 9	43 45	42 37	41 2	41 0	40 0	39 45	39 6	39 11

The mean of all the above observations, (excluding the morning observations of five and six o'clock) is 5° 40′ 34″ W. These observations show that the north end of the needle has in the morning a motion eastward amounting to from one to three minutes, when the declination is usually less than at any other hour of the day, and may therefore be called the minimum. This minimum during the winter, is attained about nine o'clock, but during the summer months commonly as early as seven. The needle then gradually deviates to the west, and attains its greatest westerly bearing about two o'clock in the afternoon, when the declination is greater than at any other hour of the day, and may therefore be called its maximum. This maximum declination is attained during the winter months, about three o'clock; and during the summer, commonly as early as one. From this time the needle again returns to the eastward, till it attains its original bearing about ten o'clock, and then continues nearly stationary until the next morning. The mean of the observations at nine o'clock in the evening, is a little less than at ten, agreeing with the results of other observers, who had remarked an evening minimum. The difference in this case is however so slight, that it might be presumed accidental.

The following table exhibits the differences between the minimum and maximum of each month. It is remarkable that the amount of this variation is less in July than in either of the preceding or following months, a circumstance which seems to have been first observed by Colonel Beaufoy, in 1818, and which was confirmed by the observations of five years. It appears somewhat improbable that such a coincidence should be accidental.

1834.			
November, - -	4′ 51″	May, - -	12′ 10″
December, - -	3 9	June, - -	11 11
1835.		July, - -	10 3
January, - -	4 27	August, - -	13 30
February, - -	2 52	September, -	14 3
March, - -	5 25	October, - -	10 39
April, - -	7 33	November, -	7 39

The following table will show to some extent how far these changes are connected with variations of temperature.

Mean temperature at each hour of the day.

1834.	5	6	7	8	9	10	11	12	1	2	3	4	5	6	7	8	9	10	Mean.
Nov.	°	44.7°	44.0°	45.4°	45.9°	47.1°	48.8°	49.6°	49.4°	49.3°	49.6°	48.4°	48.9°	46.5°	47.4°	47.5°	46.9°	46.9°	47.4°
Dec.		36.4	36.9	39.1	37.1	38.8	40.1	41.4	41.3	41.7	42.1	40.2	39.6	38.3	38.8	38.6	38.9	38.6	39.3
1835.																			
Jan.		37.2	34.4	29.8	34.1	32.7	35.5	36.6	36.6	37.7	38.2	37.5	35.6	32.0	35.2	36.5	32.9	32.0	35.0
Feb.		33.7	33.4	34.4	35.7	36.7	37.3	38.2	38.0	37.7	38.9	36.8	36.2	35.5	35.7	37.3	36.7	37.0	36.4
March,		38.8	39.6	43.9	43.3	44.6	44.5	44.7	46.5	44.8	45.3	44.8	42.5	39.1	42.4	44.2	41.0	42.6	43.1
April,		49.3	51.9	51.7	51.7	52.3	55.7	53.4	54.6	54.8	53.9	55.3	53.8	52.9	52.1	51.2	50.7	51.2	52.7
May,	68.9	68.7	62.0	63.0	63.7	64.1	63.2	65.5	65.0	68.4	63.8	66.4	66.1	63.9	61.7	63.8	64.0	63.6	64.8
June,	68.1	68.5	70.0	68.8	69.6	70.8	70.6	71.1	72.9	70.9	73.3	71.4	70.9	71.3	67.8	69.6	71.2	68.5	70.3
July,	71.9	71.3	71.4	71.8	72.7	73.3	74.9	75.5	75.9	76.3	78.1	76.3	75.2	75.7	74.1	73.3	73.1	72.5	74.1
Aug.	68.8	70.6	70.3	70.3	69.9	71.8	72.1	73.4	75.2	73.2	74.7	73.7	74.4	72.8	67.5	72.0	70.5	72.9	71.9
Sept.			60.1	60.2	62.5	63.9	62.7	63.7	65.5	64.5	68.4	64.1	66.0	63.7	64.1	62.6	60.0	64.1	63.5
Oct.			61.5	63.3	61.2	62.0	61.6	62.3	62.0	63.6	64.4	64.2	63.9	64.0	64.7	63.5	65.2	62.3	63.1
Nov.			52.8	54.5	52.4	52.3	51.5	55.1	53.2	52.9	56.3	51.9	54.6	54.1	51.6	55.8	52.2	54.2	53.5

That temperature has an influence on the amount of the diurnal variation can hardly be doubted. Thus, in November, 1834, this variation was less than in November, 1835. The thermometer indicates it to have been a colder month. The variation during the winter months, is uniformly less than during the summer months. Yet it does not appear that this variation is strictly proportioned to the temperature, for then the variation must have been greatest in July. To attempt satisfactorily to explain the cause of this diurnal variation, with the present limited number of observations, seems almost hopeless. The fact of the daily variation was first discovered by Mr. George Graham, in 1722. The discovery, however, attracted little attention until 1750, when the subject was taken up by Wargentin, secretary to the Swedish Academy of Sciences, and in 1759, Mr. John Canton, an English philosopher, made about four thousand observations on the same subject.

Since this time, like observations have been made by Van Swinden, Gilpin, Hansteen, and Beaufoy. The following table exhibits the mean diurnal variation for each month of several years, as found by different observers.

	Canton, in 1759.	Gilpin, in 1787.	Gilpin, in 1793.	Beaufoy, in 1817,8,9.
January,	7' 8"	10' 12"	4' 18"	5' 3"
February,	8 58	10 24	4 36	6 3
March,	11 17	15 0	8 30	8 22
April,	12 26	17 21	11 42	11 48
May,	13 0	18 54	10 24	9 53
June,	13 21	19 36	12 36	11 15
July,	13 14	19 36	12 30	10 43
August,	12 19	19 24	12 6	11 26
September,	11 43	15 30	9 48	9 44
October,	10 36	14 18	7 0	8 46
November,	8 9	11 6	3 48	7 10
December,	6 58	8 18	3 48	4 7

Mr. Canton first attempted to explain the cause of the diurnal variation. He established by experiment the following principle, viz. that the attractive power of a magnet decreases while the magnet is heating, and increases while it is cooling. He then assumes that the magnetic parts of the earth in the north, on the east side and on the west side of the magnetic meridian, equally attract the north end of the needle. If then the eastern magnetic parts be heated faster by the sun in the morning than the western parts, the needle will move westward, and the absolute variation will increase; but when the western magnetic parts are either heating faster or cooling slower than the eastern, the needle will move eastward, or the absolute variation will decrease. This explanation seems to account satisfactorily for the principal motion of the needle as exhibited at London, but it is not obvious how it can account for the slight easterly motion in the morning.

Mr. Barlow has adopted this hypothesis, with some modification. He observes, that while the sun is between the magnetic east and south, those parts being then most heated, their power will be diminished, and the south end of the needle ought to incline to the west, or the north end to the east, and we ought to expect that the greatest declination eastward should take place when the sun is equally distant between those points; as the sun approaches nearer the south, the parts to the west of the magnetic meridian, as well as those to the east, become heated, and the easterly deviation ought to decrease and disappear entirely as the sun passes the magnetic meridian, because then the effects on each side of that meridian are equal to each other.

Beyond this period, the southwestern parts will receive the greatest power of the solar rays, become weakened in their action, and the south end of the needle will deviate to the eastward, or the north end to the westward, and continue increasing in its deviation till the sun becomes S. W. (magnetic,) which happens between one and two o'clock in the afternoon, and its effect will be greater than the morning easterly deviation, because it happens when the sun has a greater altitude, and consequently a more intense action. From this period, the western deviation ought to diminish till the sun becomes west, (magnetic,) when it ought to cease entirely, because then the parts on the western side of the needle being equally heated, both to the north and to the south, there can be no tendency in either end to incline from the meridian.

Now the preceding theory may account for the variation of the needle at London, where the declination is about 24° W., but it does not agree at all with the observations at this place. Thus, according to his theory, the morning minimum should occur when the sun is in the S. E. (magnetic). In June this occurs with us after ten o'clock, which is about three hours after the morning minimum, as indicated by observation.

Indeed both of these theories seem much better suited to the latitude of London, than to our own; for according to either theory, the needle should occupy its mean position when the parts of the earth, both to the east and west of the magnetic meridian, are equally heated, which happens about the time, or soon after, the sun passes the magnetic meridian, that is about twelve o'clock at this place. But the observations show that the needle occupies its mean position between ten and eleven o'clock, which is about the same time as at London, although at London the magnetic meridian makes an angle with the astronomical meridian nearly eighteen degrees greater than at New Haven. The times both of minimum and maximum declination, are about the same at both places; so that as far as these observations go, they seem to prove that the diurnal variation is independent of the sun's position with reference to the magnetic meridian. It is highly desirable that these observations should be repeated in other parts of our country, particularly in the extreme Western States. The difference between the declination of the needle at London, and in Illinois for instance, is about thirty-two degrees; and although it is difficult to determine exactly the times of the minimum and the maximum, still it would seem that a difference of two hours could not fail of being detected. It surely would seem possible to determine, whether the needle occupies its mean position between twelve and one o'clock in the afternoon, as it should do according to either of the preceding theories. Some observations made by Prof. Bache, during ten days in September, 1832, exhibit results different from my own, both as to the times and amount of the maxima and minima. It is possible that these results might be modified by observations continued for a year.

The discovery that the magnetic needle was agitated during the presence of an aurora, has usually been ascribed to Wargentin. He states that on the 28th of February, 1750, the needle was disturbed by an aurora, so as to vibrate between 6° 50′ and 9° 11′ of west variation; and on April 2nd, it shifted from a like cause backward and forward, between 4° 56′ and 9° 55′. I have repeatedly witnessed a similar effect on the needle, but have never seen the effect so great as is here stated. This disturbance of the needle by an aurora is not merely occasional, but almost invariable. During the continuance of my magnetic observations, I paid particular attention to the aurora, and in every instance when the aurora was considerable, there was a palpable agitation of the needle, and almost always a deflection to the amount of ten, twenty, or thirty minutes, and in two instances of more than a degree. On the other hand, whenever the needle has experienced any unusual deflection, I have uniformly seen reason to ascribe it to an aurora. The aurora, indeed, has not always been visible; and there are several reasons why it should not be. It might occur in the day time, when it would be wholly invisible, or during moonlight, or a cloudy night, when it would be nearly if not wholly obscured. But there has not occurred an instance, during the period embraced in these observations, when the needle has suffered an unusual deflection, without an aurora being visible, unless observations were frustrated by one of the causes above mentioned. I will now enumerate all these cases, and in the chronological order:

Nov. 3, 1834.—6 h., A. M., Declination 5° 41′; 7 h., 5° 41′; 8 h., 5° 53′; 9 h., 5° 47′; 10 h., 5° 51′; 10 h. 50 m., 6° 4′; 11 h., 6° 1′; 12 h., 5° 49′; 1 h., P. M., 5° 45′. During the remainder of the day, the needle was tolerably regular, although not quite so much so as usual. Professor Olmsted observed about 8 o'clock last evening, an uncommon brightness, like the dawn, in the north, much brighter than the common aurora, which lasted with fluctuations all night. At 5 o'clock in the morning, it was nearly as bright as it had been in the evening. The needle was not observed during the evening of the 2nd; nor did I notice the aurora myself; if I had, I should have watched the needle at the same time. On the evening of the same day, a very brilliant aurora was seen in England, of which a description was given in the New York Observer of December 27, 1834. It is described as an arch of light, six or seven degrees in breadth, extending from the eastern to the western horizon, nearly through the zenith. The observers represent it as unusually splendid. See also Loudon's Magazine for 1835, p. 94.

Nov. 5.—6 h., P. M., Declination 5° 42′; 7 h., 5° 30′; 7 h. 40 m., 5° 28′; 8 h., 5° 30′; 9 h., 5° 38′; 10 h., 5° 36′. At half-past seven in the evening, although cloudy, the entire horizon, from the west point almost to the east, was lighted up like the dawn, with very considerable brightness. The brightest point, about N. 30° W.

Nov. 6.—7 h., P. M., Declination 5° 36′; 8 h., 5° 29′; 9 h., 5° 29½′; 10 h., 5° 34′. Between eight and nine, an auroral bank of light in the north-west.

Nov. 10.—7 h. P. M., Declination 5° 34′; 8 h., 5° 36′; 9 h., 5° 35′; 10 h., 5° 26′. Quite foggy. No aurora visible.

Nov. 28.—7 h., P. M., Declination 5° 37′; 8 h., 5° 32′; 9 h., 5° 37′. At eight o'clock, a faint auroral light extends along the northern horizon from the east almost to the west points.

Dec. 4.—7 h., P. M., Declination 5° 44′; 8 h., 5° 30′; 9 h., 5° 36′. At eight o'clock, a slight auroral appearance in the north; not remarkable. Aurora seen in England. (Loudon, 1835, p. 96.)

Dec. 6.—5 h., P. M., Declination 5° 44′; 6 h., 5° 31½′; 7 h., 5° 35′; 8 h., 5° 31½′; 9 h., 5° 6′; 10 h., 5° 34′; 10 h. 30 m., 5° 42′; 11 h., 5° 40′. Rainy through the forenoon; cloudy during the remainder of the day. At eight o'clock in the evening, a very evident illumination in the east. At nine o'clock, from north to east the openings in the clouds are quite luminous. At ten o'clock, the clouds broke away and showed the horizon from N. W. to N. E. to be all in a glow, a very bright and extensive bank of light. No arches or streamers. At half-past ten, the aurora fades in the east, and brightens up in the north and north-west. At eleven o'clock fades away. Vivid aurora seen in England, (Loudon, 1836, p. 36.)

Dec. 20.—6h. P. M., Declination 5° 38′; 7 h., 5° 23′; 9 h., 5° 35′; 10 h., 5° 36′. Thick clouds and the light of the moon, prevented any observations on the aurora.

	A. M.		Declination.			P. M.		Declination.	
Dec. 21.—	7h.,		5°	36′	*Dec.* 21.—	1 h.,		5°	39′
	8		6	9		2		5	39
	8	15 m.,	6	18		3		5	39
	8	40	6	37		4		5	40
	8	55	6	22		6		5	39
	9		6	7		8		5	38½
	9	15 m.,	5	54		9		5	36
	9	30	5	57		9	15 m.,	5	32
	10		5	44		9	30	5	27
	11		5	38		10		5	29½
	12		5	38					

Yesterday the air was very mild; in the evening somewhat hazy; in the night it became clear and cold—very clear all day, with a fresh breeze. At a quarter past nine in the evening, a faint aurora in the north. At half-past nine, illumination very bright directly in the north, extending about 30° in azimuth, and 6° or 7° in altitude. Mere *bank* of light. Brightest point a little west of north. At 10 o'clock, the centre of the aurora is a little east of north. Moon rose at a quarter past ten. This aurora was seen at Hanover, N. H. (Am. Jour. Vol. xxviii, p. 178,) and also in England, where it was described as most brilliant, (Loudon's Magazine for 1836, p. 33.)

Dec. 22.—Needle somewhat irregular during the whole day, particularly in the evening. 6h., P. M., Declination 5° 31′; 7h., 5° 11′; 8 h., 5° 27′; 9 h., 5° 23′; 10h., 5° 37′. Cloudy all day; in the evening, aurora very bright through partial openings in the clouds, a few degrees E. of N. Aurora very splendid in England, (Loudon, 1836, p. 33.)

Dec. 23.—5h. P. M. Declination 5° 37′; 6h., 5° 30′; 7h., 5° 29½′; 9h., 5° 29′; 10h., 5° 29′. Cloudy, yet a small spot in the N. E. horizon, at 6 o'clock, very bright, about 10° in breadth, its centre is about 15° north of Pollux. Not faded at all at seven.

Jan. 29, 1835.—7 h., P. M., Declination 5° 52′; 8 h., 5° 51′; 9 h., 5° 34′; 10 h., 5° 27′. Slightly hazy—manifest illumination; brightest point about 20° E. of north. At nine o'clock very bright in the same quarter. Seen at Hanover, (Am. Jour., Vol. xxviii, p. 179.)

Feb. 7.—6 h., A. M., Declination 6° 9′; 7 h., 5° 49′; 9 h., 5° 42′; 10 h., 5° 52′; 11 h., 5° 54′; 12 h., 5° 50′; 1 h., P. M. 5° 50′; 3 h., 5° 37½′; 4 h., 5° 38′; 5 h., 4° 56′; 6 h., 5° 40′; 8 h., 5° 37′; 9 h., 5° 37′. The needle, it will be seen, was very irregular during the day, the extreme variation being 1° 13′, but quite regular in the evening. The evening was clear and no aurora was seen, although the light of the moon would have obscured any thing but a splendid aurora. Such a one was seen in England, (Loudon, 1836, p. 34.)

Sept. 4.—2 h. 30 m., A. M., Declination 5° 22′; 2 h. 45 m. 5° 21′; 2 h. 55 m., 5° 18′; 3 h. 5° 12′; 3 h. 15 m., 5° 21′. At half-past two, a bright auroral bank of light. A streamer shoots up from north point of the horizon to γ Ursæ Minoris, about 5° in breadth; another shoots up perpendicularly to ζ Draconis. At a quarter before three, a most brilliant streamer 10° in breadth, extending up to β and γ Ursæ Minoris. Cloud stretching along on the horizon, with an arch of light extending all along upon the cloud. At five minutes before three, the streamers all moved to the east, about 6°. Highest extends up a little above γ Ursæ Minoris. This aurora was seen by Mr. E. C. Herrick, between Philadelphia and New York, from half-past twelve to three o'clock. It appeared a little to the west of north; a low arch about three degrees high resting upon a cloud; beams shot up about 30° high; moved latterly to east; brightest between two and three o'clock, when at South Amboy, N. J. Produced a very sensible illumination of the village.

	P. M.		Declination.		P. M.		Declination.
Nov. 17.—	7h.,		5° 52′	*Nov.* 17.—	11h.,	19m.,	5° 52′
	8		5 37		11	21	5 42
	9		5 52		11	24	5 37
	10		5 52		11	27	5 22
	10	55 m.,	5 17		11	30	5 12
	11		6 22		11	34	5 22
	11	4 m.,	5 37		11	44	5 37
	11	6	5 36		11	52	5 25
	11	10	5 47		11	59	5 37
	11	14	5 52		12	9	5 45
	11	15	6 7				

	A. M.		Declination		A. M.		Declination.
Nov. 18—	7h.,		6° 53′	*Nov.* 18.—	9h.,		5° 36′
	7	30m.,	6 38		10		5 53
	8		6 35		11		5 52
	8	30m.,	6 2		12		6 7

This was the most remarkable aurora I have ever witnessed, and the most remarkable disturbance of the magnetic needle, the entire range being 1° 41′. A particular account of the appearances has been given in this Journal, Vol. xxix, p. 388. The needle was little, if at all affected by the auroral arch which appeared during the forepart of the evening, but was very violently affected by the crimson columns which formed about eleven o'clock. It is doubtful whether I observed the greatest agitation of the needle at this time, for I did not commence my observations until the corona was almost completely formed. This auroral arch was the only instance observed during the year, of an arch completely spanning the heavens. As very careful observations were made upon it at Dartmouth College, which lies almost due north from New Haven, at a distance of about one hundred and sixty-four miles in a right line, we have the materials for calculating its height. This arch at Dartmouth College, appeared in the south at eight o'clock, having an altitude of 38°. At New Haven, at the same time, its altitude was 75°, from which we at once obtain its height to be about one hundred and sixty miles.

On the evening of Nov. 18th, there was a slight repetition of the aurora. A diffuse light was spread all along the northern horizon, and rose to a considerable elevation. The appearances, however, were at no time splendid. I was absent from my room during the principal part of the evening, and could not therefore observe the needle constantly; yet at seven, eight, and eleven o'clock, the needle was as regular as usual.

In England an aurora was observed on the night of the 17th, and early in the morning of the 18th, and so much did the appearance resemble a natural fire at a distance, that we are told at London, 'sixty men and twelve fire engines hastened towards some dreadful conflagration.' About midnight, clouds intervened, and the fire became extinguished, but the aurora again burst forth about 3 A. M., so that the firemen were again on the alert. On the evening of the 18th, the aurora was uncommonly splendid, consisting of beams and coruscations which shot up to the zenith. The light was, however, almost entirely white. (Loudon's Magazine for 1836, pp. 23—36.)

The preceding catalogue contains all the instances in which the aurora was observed here during the year, and also all the instances in which the needle was decidedly irregular. These observations lead us to the conclusion, that auroras are most common during the months of November and December. That when the aurora consists merely of a *bank of light* like the dawn, and rises but little above the horizon, the disturbance of the magnetic needle is very little, and is generally proportioned to the vividness and extent of the aurora. The needle has sometimes appeared to veer towards the point of greatest brightness, and sometimes to recede from it. This is a question which deserves more consideration.

Auroral *beams* cause a disturbance of the needle, at least, when the beams are themselves in active motion.

Auroral *waves or flashes*, when rising to the magnetic pole, cause a violent agitation of the needle, which consists of an irregular oscillation, sometimes to the amount of nearly a degree, on each side of its mean position. When the aurora ceases, the needle soon returns to its former state.

An auroral *arch* has little, if any, influence on the magnetic needle.

During snow storms and thunder storms, I have commonly observed considerable *agitation* of the needle, like that arising from a shaking of the whole building, but have never seen any *deflection* of the needle. No great weight, however, can be attached to this observation, for it is by no means uncommon for the needle to shake with a very tremulous motion, even when there is no agitation of the building, and no perceptible cause for the disturbance.

XLVI. *Notes on Chemistry, &c.* By J. W. BAILEY, *acting Prof. Chemistry, &c., U. S. Mil. Academy, West Point.*

1. *On a new test for Nitric Acid.* Chemical re-agents may be divided into two classes: first, those which produce with the substance they are employed to detect, an action which they will produce with no other known body; an example is starch, as a test for free iodine: secondly, those which cause a certain action with a *small number* of bodies, which they will not exhibit with any others; as, for example, sulphuretted hydrogen, which causes a black precipitate with a *few* metals.

The first class are, of course, the most valuable re-agents, as they require no subsequent operation to determine whether certain substances are present or not; while with those of the second class, we only determine that one of a certain number of bodies must be present, but must then resort to other means to ascertain which particular one it may be.

There are many cases, however, when we may know that only one of those bodies which are capable of giving similar results with the re-agent added, is present, and then if this result is produced, the evidence is as satisfactory as can be desired.

The test which I would propose, must be placed among those of the second class, and is therefore inferior in value to morphia as a re-agent for nitric acid; but I think it *at least* as valuable as the method by means of gold leaf and hydrochloric acid, or by the bleaching of indigo.

The substance I now suggest as a new re-agent for nitric acid, is the cyano-hydrargirate of iodine of potassium, discovered M. Carllot. It is formed by mixing together bicyanuret of mercury and iodide of potassium, (one equivalent of each) dissolved in small quantities of warm water. It soon crystallizes in a very beautiful manner. This is the same salt which has recently been recommended as a means of detecting the presence of hydrochloric acid in hydrocynic acid. (See Lon. and Edin. Phil. Mag., Nov. 1835).

Its use as a test for nitric acid depends upon the fact, that if one of the scale-like crystals be introduced into *most* acids, it immediately becomes of a beautiful *red*, being changed into the bi-iodide of mercury; while in *concentrated* nitric acid, (spec. grav. 1·4 to 1·5) the scale instantly becomes almost black, from the liberation of iodine. A scale of the salt introduced into a drop of the acid no larger than a pin's head will show the effect distinctly.

The acids in which I have found this salt to *redden*, are, sulphuric, hydrochloric, hydrofluoric, chromic, phosphoric, (if slightly diluted) and common vegetable acids, such as oxalic, tartaric, citric, and acetic acids.

I have found it to blacken with chlorine gas, solution of chlorine, (recently prepared) bromine, sulphuretted hydrogen, nitrous acid vapours, and *nitric acid*.

It is highly probable, that it would be blackened by bromic acid, and chloric acid, and possibly by iodic acid, but I have not at present these acids in a free state to determine their action; the method, however, in which I use the test will prevent any fallacy from the presence of chloric, bromic, iodic, or chromic acids, and of sulphuretted hydrogen. It is to evaporate the supposed nitrate to dryness, and introduce into a tube retort A fig. 66, Plate VIII., a small portion of the salt, on which a few drops of sulphuric acid are to be poured; then on applying moderate heat, by means of a spirit lamp, a portion of the volatile products are to be driven over into the receiver B, in which a few scales of the salt are previously placed. If these are blackened, the salt is to be considered as a nitrate, provided the presence of those few substances which might cause the same result has been guarded against. Now by the very method proposed, viz. evaporating to dryness and adding sulphuric acid, the presence or absence of chronic, chloric, or iodic acid, and sulphuretted hydrogen, will be determined: for the colour of a cromate, the evolution of peroxide of chlorine from chlorate, the liberation of iodine from an iodate, and the odor from a sulphuret, will at once decide with regard to each. As iodic and bronic acids, even if they are found to blacken the salt, are not sufficiently volatile to be driven over by the heat to be employed, no error could arise from their presence.

I have observed, that if the salt used above, or the bi-iodide of mercury itself, be introduced into a test tube, with strong sulphuric acid, on adding a concentrated solution of any nitrate, (except those of silver and mercury) the red colour of the scale or bi-iodide will speedily disappear, and will be followed by the dark hue of iodine. Even when the sulphuric acid forms an insoluble precipitate, the action may be seen, by stirring up the precipitate with a glass rod, when the dark spots will be easily observed.

This method of testing may sometimes be used, but is liable to the objection that a chromate, chlorate, and probably some other salts, would give the same result. It is greatly inferior to the method by distillation, as given above.

Silliman's Journal.

XLVII. *Davenport's recent experiments in Electro-Magnetic Machinery.*

(Copy of a letter from Mr. Davenport.)

To Professor Silliman.

Dear Sir.

Having lately made a number of applications of the power of *large galvanic magnets* in propelling machinery, (being independent of the large machine now constructing by the association,*) I have thought proper to state to you the results, believing they would not be uninteresting to you.

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* The machine alluded to in the above letter, as now being constructed for the Electro-Magnetic Association, by Messrs. Davenport and Cook, is nearly completed, and is expected to be of about two tons' power. It is formed by a combination of small magnets, weighing about four pounds each, and three and a half inches between the poles. These magnets are placed, two hundred and thirty four in number, on an iron shaft six feet in length, and a corresponding number in a circle as stationary magnets.

I have constructed a machine, with two revolving magnets, two feet in length, made of iron three and a half inches in diameter, and weighing, after being wound with six coils of No. 10 copper wire, one hundred pounds each. Three stationary magnets of two feet diameter, were placed around the periphery, making six poles, and weighing one hundred pounds each.

With this machine I produced one hundred revolutions per minute, with six square feet of sheet zinc exposed to action, surrounded with thin sheet copper.

I then displaced the stationary magnets, and substituted one magnet three inches in diameter, forming a semicircle, with the poles directly opposite each other, and weighing about one hundred pounds. With this magnet I produced one hundred and fifty revolutions per minute, using the same quantity of zinc surface. With one revolving magnet I produced one hundred and seventy-five revolutions per minute, with four square feet of sheet zinc. I next constructed a *hollow* magnet of two feet in length and four inches in diameter, made of boiler iron, five-sixteenths of an inch in thickness, with four coils of copper wire, with which I succeeded in getting one hundred revolutions per minute. A hollow magnet was then constructed of thin sheet iron, of the thickness of common *stove-pipe iron*, which revolved one hundred and fifty times per minute. *Hollow* magnets I think may be used to great advantage where weight is an objection; but in my experiments I generally make use of *solid* iron.

I also constructed a machine with simply two magnets formed of two inch round iron, of fifteen inches in length, of the stirrup form. The distance between the centres of the poles is five inches, and the magnet revolves four hundred and fifty times per minute, with two square feet of zinc. The stationary magnets being placed with the poles pointing upwards, and the poles of the revolving magnet pointing downwards, the shaft to which the revolving magnet is attached passes through its centre, and rests on the centre of the stationary magnet. Two of these machines (weighing in all fifty pounds) I have attached to small drilling-works, which I find produce sufficient power to do all my drilling of iron and steel, to the size of one-fourth of an inch in diameter.

I have adopted this form on the third machine which I have recently put in operation. The magnets are formed of two and three-fourth inch iron, with the centres of their poles nine inches apart and weighing fifty pounds each, with this I produced three hundred revolutions per minute, and have successfully attached it to turning hard wood of three inches diameter.

I find the power increases in full proportion to the increase of weight and without increasing in proportion the size of the battery. The wire must be increased in size in proportion to the size of the iron used, and, consequently, the difficulty attending long wires will always be avoided.

I find no difficulty in using my machine *twelve hours in succession*, without changing batteries or agitating the solution.

I am erecting conveniences to test the powers of each magnet as they are increased in weight and size, and think I shall be able in season for the April number of your Journal to give the exact increase of power in proportion to weight, of magnets weighing from ten pounds to several tons.

I have also made some very satisfactory trials, while making my machines, respecting the expense for the consumption of zinc and acids, and I think I shall soon be able to give nearly the precise cost of making the largest machinery.

Galvanism is, I trust, destined to produce the greatest results in the most simple form, and I hope not to be considered an enthusiast, when I venture to predict, that soon engines capable of propelling the largest machinery will be produced by the simple action of *two galvanic magnets*, and worked with much less expense than steam.

Yours, respectfully,
THOMAS DAVENPORT.

New York,
December 26, 1837. *Silliman's Journal.*

XLVIII. *Rotary Multiplier, or Astatic Galvanometer.* *By* Charles G. Page, M. D.

Figures 65 and 66 represent two new pieces of galvanic apparatus, completed in the beginning of September last. Fig 63 represents a rotary or astatic galvanometer, with a single needle. *m* is the multiplier, composed of a number of turns of insulated wire. At *c*, an open collar passes through the centre of the wires, to prevent any friction against the stem supporting the bar magnet *n s*. The multiplier *m* is mounted for revolution on a slender shaft *b*, and has the ends of its wires soldered to semi-cylinders of silver at *a*, upon which the battery wires *p n* press with a slight spring. The cylindrical segments are not correctly represented in the figure. Their relation to the battery wires should be such that the direction of the current should change when the coil of wire is at right angles to the magnet. The magnet being stationary may be very large and powerful, and the mode of suspending the wire allows it to be brought much nearer the magnet than is represented in the figure; at the same time the friction is trivial. The apparatus would be much improved by the addition of another bar magnet n' s' above the coil. In that case the magnetism of both bars might be preserved, by arming their opposite poles when not in use. This instrument, though interesting, is not intended as a measurer of galvanic force. But the principle of making the conducting wires the indicators instead of the magnets, appears to be of value, for many reasons. The conducting wires may be considered as perfectly astatic, and affording constant results. It is difficult to obtain, and much more so to preserve a perfectly astatic needle. The needle of a galvanometer is readily disturbed by the approach of any ferruginous body. By substituting for the dissected cylinder at *a* two entire cylinders above and below, the rotary multiplier becomes a galvanoscope of considerable delicacy. In order to constitute an astatic galvanometer the whole should be inverted, the magnets supported from below, the multiplier by a torsion thread from above, and the extremities of the wire turn in small mercury cups in the centre of motion.*

Fig. 64, represents a new form of electrepeter. Its name purports a turner or changer of the electrical current. An ingenious apparatus of this kind, by Mr. Clarke of London, is described and figured in the first number of Sturgeon's Annals; but it is not so simple in construction, and the connecting wires in his machine being out of sight, it is not so easily understood as the one here figured. The drawing represents a double electrepeter, or one to be used with two separate batteries, and two or more pieces of electro-magnetic apparatus. Divide the machine at *a*, and bring up the wooden pillar *r*. to the left hand division, and you have the single electrepeter, answering for most purposes. The one I have constructed, was made for reversing the motion of an electro-magnetic engine, and has four parts. *a c*, is a cylinder of mahogany three-fourths of an inch diameter, mounted for semi-rotation between two wooden pillars. *b b b b* represent strips of silver passing each obliquely half round this cylinder, and fastened to it by pins of the same metal. b' b' b' b' represent rectangular studs of silver, (copper will answer, but not so well) connected metallically through the centre of the cylinder with corresponding studs directly opposite. *p s*, *q s*, are stiff springs of copper, with silver tips at *s* pressing firmly against the studs on the cylinder, and connected with the mercury cups below. The back side of the instrument exactly corresponds to that exhibited in the drawing. The modus operandi is seen at a glance. The two springs *p n* are connected by the mercury cups with the poles of a battery. The corresponding springs of the other side, with the wires of an electro-magnet for instance. By turning the cylinder half round, it is obvious the battery current is crossed, and the poles of the magnet reversed.

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* The wires here should be of silver, except the tips for the mercury connexion.

New form of interruptor or electrotome.—As it is desirable that every distinct form of apparatus of general use should have an appropriate name, I have selected the term electrotome (divider of the electrical current) as applicable to the several varieties of apparatus figured and described in the July number (1837) of your Journal. It is hardly necessary to premise, that secondary currents of great intensity, are obtained from a single pair of plates in connexion with the dynamic multiplier, when the primitive current is divided in any part of its course. The force of the secondary current so obtained, depends materially upon the mode of breaking the circuit of the primitive. The shocks and decompositions I have found to be greatest when the primitive circuit is broken by raising clean pencils of lead, zinc, or copper, from the surface of mercury covered with water. The sparks are best exhibited over clean mercury with lead. The mechanical electrotome I have contrived with a view to combine the above advantages, at the same time that it is a useful instrument, affords a most brilliant exhibition of galvanic power. The connexion is rapidly broken by a long series of leaden bars, raised from the surface of mercury in succession by pins arranged at proper distances on a revolving metallic drum, similar to that of a barrel organ or musical box. The lead bars, or wires, of large size, are supported in a wood frame by projecting shoulders, to take the pins of the drum as it revolves. Their lower ends just dip into the mercury of a long narrow cell with glass sides, The drum is connected with the battery by a strip of copper pressing firmly against its metallic axis. The mercury in the cell is connected with the spiral by a wire. As the pins come round in successive order, they establish the battery connexion, and again break it by raising the piece of lead, and so each one in order. Revolved by a multiplying wheel the effect is exceedingly beautiful, while, in the dark, illuminated by its own light, the whole appears to be at rest.

Fig. 62, plate VIII, is a plan for exhibiting the polarity and curious motions of De la Rive's ring, floating in the air instead of acid and water. The great advantage of this construction, and that of fig. 29, plate IV,* is that the ring and helix, and the batteries, may be of any desired size. *a*, represents the ring, suspended as in fig. 28, plate IV. The wire ends descend into concentric mercurial cells *c, c*. These separate cells communicate with the battery cells *d, d*, by wires passing along the slight lever beam *e*. The ring and cells are balanced by a small weight *b*. The magnet *m*, supported by its centre, is bent so as to form an arc of the circle described by the ring. Suppose the ring to be in equilibrium at the neutral point of the magnet *m*. Reverse the battery wires in the cells *d, d*, and the ring starts off from the bar, turns round, presents its other face, and passes on to *m*, as before; so that this motion can be produced on a large scale at pleasure, simply by changing the battery connexions without disturbing the magnet, as in the floating apparatus. All this can be done with solid conductors, but there being no rapid motion in this experiment, the mercury cells are preferable, from their simplicity.

Silliman's Journal.

Note.

By comparing Dr. Page's instrument, fig. 63, with that shown at fig. 31, Plate IV., and described at page 144, they will be found to operate on precisely the same principle; which, as I have already shown (page 145), is very far from being new. Dr. Page uses a voltaic battery to put his coils into motion. I have already stated that the rectangle, fig. 31, rotates by a thermo electric current. I have, however, used batteries for this purpose, in which case the magnet is not needed, as the magnetism of the earth is quite sufficient to deflect the rectangle, or coil, so as to change the connexions, which is all that is wanted to produce rotatory motion. When no magnet is used, it is necessary to place the partitions which separate the mercury cells, and where the battery connexions change, at about right angles to the magnetic meridian. I have already stated, page 145, that I have found oil of great service in electro-magnetic, and magnetic electrical machines, and that I have used it for a long time. Dr. Page first mentions his employing oil, for these purposes, in his paper dated August 15th, 1837 (see page 216 of this volume), hence, in order to convince that gentleman that he was anticipated in this country, it would only be necessary to refer to the date of Dr. Hare's visit to England. That eminent philosopher, accompanied by his two sons, honoured me with a visit at my then residence at Woolwich, in the Summer of 1836. I showed them several pieces of apparatus, two of which were magnetic electrical machines, the one *without* iron, described in the first article of this volume; the other *with* an iron armature, in both of which, *oil was used,* to lubricate the springs and revolving discharging piece, fig. 6, Plate I. Dr. Henry, also, in the Summer of 1837, honoured me with a visit, and saw the same machines. Dr. Page's paper, in which he describes his first use of oil, was published in Silliman's Journal, for October, 1837, which did not arrive in London till nearly Christmas.

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* The description of fig. 29, is given at page 143 of this vol. Edit.

XLIX. *Physical, Chemical, and Physiological researches on the Torpedo.* *By* M. CHARLES MATTEUCCI.*

"If it ever be discovered that the electric fluid intervenes in the phenomena of life, it will be by studying the singular properties that certain fish possess of giving, when touched by the hand, a shock similar to that of the Leyden jar.

These very profound words of one of the greatest philosophers of our time, served only to confirm an opinion that I had already started in my first memoir on the torpedo, read at the Institute, July 11th, 1836. Speaking of the body of the torpedo I have said at the end of this memoir, we shall very probably see this, even yet undetermined, great secret of organic life make its appearance.

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* This memoir lately read by the author at "l'Academie des Sciences de Paris," and the priority of whose publication he much wished to offer us, appeared to treat the subject of the electricity of the terpedo in so complete a manner to comprise in itself such interesting results, that, notwithstanding its length we did not hesitate to insert it in our collection. Although it would be difficult for us, in a work comprising so vast a field of sciences, to find the necessary space to publish many special memoirs; we shall, however, continue to make an exception in favor of electricity, and some parts of philosophy and chemistry which are connected with it, having seen the great interest actually excited by this branch of the physical sciences, and the great number of original works relating thereto that are frequently addressed to us. *(A. De la Rive).*

Communicated by the Author. EDIT.

† Translated by Mr. J. H. LANG.

Continually harassed by these thoughts, and supported by the hope of arriving at the aim of my researches, I spared nothing to obtain success. Two months, June and July of the present year, spent on the shores of the Adriatic have furnished me with 116 living torpedoes of different sizes. I have even gone myself in small boats to fish for them and thus enabled myself to study this fish in all its vitality. I flatter myself that all these labours have not been lost and that the general physiology and history of this fish owe some new light to my researches. I have tried to study these animals under all circumstances; I have interrogated the fishermen to find out their motions; I have obtained the discharge when they were scarcely out of the water; I have analysed the air of the water in which I have made them live, obliging them to give strong discharges; I have examined the action of heat, of the electric current, of different gaseous substances, poisons, &c., upon them; all of which have formed the subjects of long researches.

I then considered that it was necessary to place the materials of this work in a certain order. But above all, I ought to relate in a few words the history of the discoveries made on the torpedo, so as to determine precisely the real state of our information. I shall not make it as extensive as might be expected: I am prevented from so doing by the want of a complete collection of all the journals and works on natural history which I should require.

Besides, you will find a very long chapter on this subject in the great work of M. Becquerel.

CHAPTER I.

It has long been known that the torpedo, while living, gives shocks, when touched by the hand, on the back and belly at the same time. This property has given it the common name of *tremble, magic fish, &c.* It is even known among fishermen, that the torpedo gives the shock voluntarily in its own defence and to kill the other fish on which it is nourished. They even indicate the great force of this shock by saying that it is sufficient to kill pollards, which are the most hardy and lively salt-water fish in our regions. We are indebted to M. M. Humboldt and Gay-Lussac, for the first researches on the electric nature of this shock, and general laws of the discharge. The Italians, Redi and Lorenzini, first studied this fish, in regard to its anatomy, and above all the position of the electric organ. This work has been followed up in all the electrical fish by Hunter; and Geoffrey St. Hilaire, Galvani, and Spallanzini, discovered also the influence of the nerves of the brain and the circulation of the blood on the discharge of the torpedo. The most important work on the torpedo that has been published of late is that of John Davy, brother to the celebrated chemist. It is to him we are indebted for the discovery of the action of the current of the torpedo on the magnetic needle, its magnetizing power, and its electro-chemical action.* Messrs. Becquerel and Breschet have also made researches on the torpedo in the year 1835. It is to the first of these two philosophers that we are indebted for the very exact means of studying this current—it was he, who fixed precisely the direction of the exterior current—as to the second of these philosophers, we are impatiently awaiting the publication of his anatomic works. Indeed, last year, I proposed applying Faraday's extra current apparatus to the current of the torpedo to extract a spark from it. I informed M. Linari, of Sienna, of this apparatus, with the modifications it requires for the end in question, and we have both separately obtained the spark, in the discharge of the torpedo.† I also discovered and published at the same time several physiological facts, such as the action of certain poison, the discharges after death, the action of the last lobe, &c. M. Colladen confirmed my researches in a work compiled at the same time, and afterwards suggested some ingenious ideas on the production of this electric discharge. M. Linari also in the month of August, of the same year, obtained a spark from the torpedo without having recourse to the apparatus I proposed.

CHAPTER II.

I shall briefly describe the principal apparati I employed in my last researches on the torpedo. The first were galvanometers constructed like the model devised by M. Colladen. I had one, in particular, which was sensible enough; the copper wire, one quarter of a millimetre thick, had a double coat of silk, and was also covered with a coat of gum-lac varnish. The wire had six hundred turns round the astatic needle. At the extremities were soldered two plates of platina. Although the wire was well insulated, I never obtained more than feeble traces of the current from the discharge of a small Leyden jar. A galvanometer, similar to that I have just described, is the best for studying the discharge of the torpedo. More delicate, that is to say, with a greater number of turns, it begins to be sensible to the electro-chemical actions of the platina plates and the secondary polarities; and if we oblige the current to traverse a bed of water, we run a greater risk of stopping the current of the torpedo, than that of the eletro-chemical origin. The other electroscope which I very often employed, was the frog prepared in Galvani's style. I even succeeded in using it to determine the direction of the current; for this purpose I cut the frog where the two thighs meet, and caused the electric discharge to circulate from one foot to the other. If the frog be a little enfeebled, it is always the thigh by which the current goes out of that is agitated when the current passes. The apparatus, by the aid of which I now obtain the spark, will be described when I speak of this phenomenon.

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* This work, which appeared in 1832, is particularly important for the anatomic and natural history division.

† The discussion concerning the priority of the discovery of the spark, which took place between M. Linari and myself, obliged me, against my own will, to show the commissaries of the Institute the correspondence which took place on this subject between the doctor of science and myself.

CHAPTER III.

On the phenomena of the electrical discharge of the Torpedo.

Every time a living torpedo is taken in the hand, it is not long before a strong shock is experienced from it, which may generally be compared to that of a pile, of 100 or 150 pairs, charged with salt water. This force becomes much more feeble after a certain time, even though the animal be kept in vessels of salt water. These discharges succeed one another with great rapidity, when the animal is quite well, and are then almost insupportable. The following observation, which is common among the fishermen, and which I have myself verified, will suffice to give some idea of them. When they draw up their nets and turn the fish into the boat, they begin washing them, by throwing large quantities of salt water over them: they then perceive in a moment if they have a torpedo, by the shock which is felt in the arm that throws the water. If you then take it up in your hand to try it, the shocks that it gives are so violent and rapid that it becomes necessary to drop it, and the arm is for some time benumbed. After a little time it ceases to give any; but you are sure to receive one the moment you replace it in the water. Some very slight motions may be seen in the body of the torpedo when it gives the electrical discharge. I ascertained by a very simple experiment that it could discharge itself without its body undergoing any change in size. I placed a middling sized female torpedo 0^{m}

14 wide, in a vessel full of salt water, and with it a frog prepared and put on its body. The vessel was perfectly closed and had a tube of very small diameter. After having carefully stopped the mouth, I finished filling the vessel with water, so that the liquid was raised in the small tube. The torpedo gave, from time to time, discharges in a particular manner, which I shall hereafter describe; the frog contracted itself: but the level of the liquid in the small tube was immoveable.

When the animal is endowed with a great vitality, the shock is felt, whatever part of the body be touched. In proportion as the vitality ceases, the region of its body in which the discharge is perceptible is reduced to that which corresponds to the organs commonly called electrical.

I am convinced, by experiments, that the torpedo has not the power of directing the discharge, where it likes or where it is irritated. It can discharge itself, *when*, but not *where*, it pleases. It was thought that it could direct its discharge as it wished, because the shock was felt in that part of the body which touched the torpedo, and because the irritated part of the fish is that on which it is touched: but the following is what takes place. If the discharges are strong, the animal being in full vigour, they are experienced at whatever part the torpedo is touched. When it is weakened, and you irritate it to obtain the discharge, it is no longer in all parts of its body that you perceive it. I have placed several prepared frogs, on different parts of the body of a very feeble torpedo: I have irritated it with a knife, at the tail, fins, gills, &c. The frogs which leaped were, in every case, those that I had placed on the electric organs.

By means of the frog alone, I have been able to establish what was, during the discharge, the distribution of the electricity over the body of the torpedo. In order that the frog, or any body, be traversed by the electric current of the torpedo which discharges itself, it is always necessary for them to be touched by it in two different points. If, for example, we take a frog, to which a single string of the crural nerve has been left, and that we afterwards touch the torpedo with the only extremity of this nerve, keeping the frog *insulated*, we never perceive the latter contracted, while other frogs placed on the fish undergo very great contractions. In order to see the *insulated* frog contracted, by the discharge of the torpedo, we must touch it by two nervous strings, or a nerve and a muscle: in short, two points of the frog must touch two of the torpedo. If the frog be not sustained by some insulating body, but, on the contrary, communicates with the ground, we then perceive it contract itself, even when it touches the torpedo, only by the single extremity of a nervous thread.

With the galvanometer, the distribution of the electricity is very easily determined. It is only necessary to bring the platina plates of the galvanometer, on the different parts of the electric organ. When we require comparable and exact results, it is better to destroy one of the organs, which is done either by cutting it entirely out, or only the nerves. I then make the experiment on the remaining organ untouched, without fearing the discharge of the other impeding that which we are studying. The following are the general laws of this distribution.—

1st. All the points of the dorsal part of the organ are positive, with regard to those of the ventral part.

2d. The points of the organ on the dorsal face, which are above the nerves, penetrating this organ, are positive with regard to the other points of the same dorsal face.

3d. The points of the organ on the ventral face, corresponding to those which are positive on the dorsal face, are negative with regard to the other points of the same ventral face.

These three laws, which are founded on a great number of experiments, clearly explain all the cases of the current, which arise from touching either a single face of the organ in two different points, or even the two organs at once on the same face, provided that the points touched be not symmetrical.

I have also determined in what manner the current moves in the act of the discharge, from the exterior skin to the interior of the organ. For these experiments I covered my platina plates with varnish, leaving only a very narrow band uncovered. The organ was cut horizontally, and the two interior faces separated by a plate of glass; or cut vertically and the platina plates more or less deeply introduced. I varied these dispositions in every way, and the general result was always the following:—The positive plate of the galvanometer was always that touching the dorsal skin, or that which was nearest to this part, with regard to the plate touching the ventral skin, or the interior part of the organ which is nearest to this skin.

On examining the intensity of the current, by the galvanometer, I found that it varied according to the extent of the plates touching the two faces of the organ.

I wished also to examine what was the nature of the current of the torpedo, when made to pass during a greater or less time through a bed of salt water, or through this same bed divided by a metallic diaphragm. The general principle that

I discovered is this: when the torpedo is endowed with great vigour, at the time it is just taken out of the sea, the current that it gives may be compared to that of a pile of a great number of pairs, and charged with an active and good conducting liquid. In proportion as its vigour is lessened, the current of the torpedo always approaches that of a feeble pile, and always of a less number of pairs. To stop at a deviation of the galvanometer that might be comparable, I proceeded in the following manner.—I placed the torpedo, just taken out of the water and dried, on an insulated metallic plate. It is the plate of the apparatus that I shall describe hereafter, and which I used to produce the spark. Another metallic plate having a glass handle was placed on the torpedo. Copper wires were soldered to these plates, and were connected where necessary. To obtain a fixed deviation, I irritated the torpedo, placed as I have said, so that it gave eight or ten successive discharges, and I take the final deviation at the half of the oscillations. I afterwards take away the torpedo, immerse it again in the sea water, and at the end of six or eight minutes I re-submit it to the experiment, and so on. On a very lively female torpedo, 0^{m} 18 wide, I made the following experiment.—By establishing a complete metallic circuit I had a deviation of 80°. This same current afterwards passing through a bed of salt water, 0^{m} 40 long, very wide and very deep, introduced by platina electrodes, 6 centimetres square, was scarcely enfeebled; the same torpedo gave me, after some time, 50° with the entire metallic circuit, and 12° with the addition of the bed of salt water. The current of another torpedo already weakened, gave 30° in passing over the metallic wire, and 6° in passing through the bed of salt water, 0^{m} 20 long, wide and deep 0^{m} 02, in the middle of which was a platina diaphragm. This same torpedo still more enfeebled gave 12° in the first case and scarcely any traces of electricity in the second.

The phenomena of electro-chemical decompositions, already obtained by John Davy, have been studied by me. I shall only mention one very simple manner of producing them. It consists in forming the circuit between the two faces of the organ, with a band of paper moistened in a very saturated solution of iodine of potassium. Two platina plates are interposed between the surfaces of the organ and the edges of the paper. After some discharges the indications of the decomposition appear.

The electric spark is very easily obtained with the apparatus I have described. Gold leaves are fixed with gum on the two metallic balls. These two leaves are held at the distance of a demi-millimetre, and by lightly moving the upper metallic plate, the animal is irritated, at the same time the leaves move, approaching and diverging almost simultaneously. Very brilliant sparks are seen to shine between the gold leaves.

CHAPTER IV.

On the exterior and interior causes which influence the discharge of the torpedo.

By exterior causes I mean those which do not sensibly destroy the organization of the fish : and vice versâ for the interior causes. I shall demonstrate them in two distinct sections.

1*st. Section—Exterior causes.*

The life of the torpedo is prolonged more or less by the three following circumstances: 1st. the quantity of sea water in which it is kept; 2d. the temperature of this water; and 3d. the degree of irritation to which the animal is submitted, and by which it is obliged very often to discharge itself. I have succeeded in prolonging the life of the torpedo for three days in my room, by uniting in a manner very favourable to the animal, the three circumstances above mentioned. It must, however, be observed, that the causes which prolong the life of the torpedo, are not the same which increase the activity of its electric function. We shall see in this section that the electric function, and the prolongation of the life of the animal, vary, by the effect of the same causes acting in an opposite manner. We will first speak of heat.

In a quantity of water, about a metre deep, and contained in a vessel 30 centimetres in diameter, the temperature of which was +18° R, the torpedo usually lives only five or six hours at most, always preserving its electric force with a greater or less degree of activity. If the temperature be lowered the electric function ceases almost at the same time. I took two female torpedoes, caught at the same time, and of a middling size. I commenced my experiment three hours after I caught them. They were placed in equal quantities of sea water, but of different temperatures, the one being +18° R, and the other +4° R. At the end of five minutes, the torpedo immersed in the cold water gave no more electric discharges, nor any motion, though irritated. In five minutes more, scarcely any motion was perceptible in its gills; it might have been thought dead: the other torpedo was just in its usual state. I took the former out of the water and placed it with the other. In about ten minutes it had regained its former force, in every respect like the other. I repeated this experiment four successive times on the same fish, and always with the same result, excepting that it required as much more time to recover itself as it had been longer cooled. I saw a very small male torpedo removed by night for ten hours in a very small quantity of sea water, at the temperature of about +8° or 10° R; it arrived benumbed and almost dead. The state in which I saw it made me take it from the water and place it upon a table, on which a ray of the rising sun fell. I then saw it move; I placed it in some water at +16° R; in a moment it gave me an electrical discharge, and continued to live for about an hour. I studied the action of renewing the heat on another torpedo; it was a female one and not very active. I placed it in some sea water that I could heat at pleasure. In proportion as the heat increased, I took care to touch the animal, which never ceased giving strong electrical discharges. The temperature was at +30° R, when the animal gave me five or six electrical discharges stronger than before, which lasted some seconds, after which it died. I prolonged the existence of another torpedo in water at +26° R, which continued to give discharges, but soon died. If care be taken to remove it immediately from water of about +24° or 26° R, and place it in water at about +18° R, it may be recovered. This is an experiment I repeated several times. We can very easily explain this action of heat without having recourse to unknown causes or too distant analogies. The principles established in the great works of Edwards, on respiration, are sufficient to elucidate this phenomenon. There remains only to admit that the electrical function is proportional to the activity of the circulation and respiration of the animal. The fish immersed in cold water has its circulation almost instantly stopped, and a small quantity of air suffices to maintain its benumbed existence. In warm water, the circulation and respiration become very rapid, but the fish soon dies on account of the diminution of the air, the quantity of which is no longer in proportion to the additional activity of these two functions.

Before entering upon the study of the respiration of the torpedo, with regard to its electrical function, I ought to commence with the analysis of the air dissolved in the sea water. My apparatus was the same as that employed by M. de Humboldt, in his celebrated work on the respiration of fish. The analysis of the air was made by potassium and the combustion of phosphorus. I repeated this analysis several times, and observed great differences in the results, according to the parts of the sea whence the water was taken, and according to the temperature at which it was exposed. I shall here give the mean composition of the air, contained in the sea water, near the coast of Cesenatico, at 13° R and one foot below the surface, 3500cc of water gave me 62.5 of a cubic inch, English, equivalent to 101.87. The composition for 100 of this mixture, was, 11 of carbonic acid, 60.5 of azote, and 29.5 of oxygen. This composition was constant as far as regards the oxygen and azote; the carbonic acid varied from 0.08 to 0.27. The same sea water taken near my house, in a small reservoir which opened into the canal of the port, at the temperature of +22° R gave me the following composition: 3500cc gave 45 tenths of a cubic inch, English, of which the composition of 100 of the mixture was 17.8 carbonic acid, 24.4 oxygen and 57.8 azote. We will now notice the change made in this quantity of air and in its composition by the respiration of the torpedo. I made two experiments choosing two female torpedoes of almost equal vigour and size. One of these torpedoes was immersed in the water, of which I have given the analysis; it was quiet for 45 minutes, at the temperature of +22° R: the other torpedo was in the same condition, except that it was continually obliged to give its discharge. Having taken them still living out of the water, I passed immediately to the analysis of the air contained in these two separate portions of sea water, of which the following are the results:

Air of the water of the torpedo which gave the discharges.

3500cc gave 30·5 tenths of a cubic inch, English.

	Composition.	
Carbonic acid	11	30·6
Azote - -	19·5	69·4
Oxygen - -	some	traces
	30·5	100·0

Air of the water of the torpedo which remained quiet.

3500cc gave 33·75 tenths of a cubic inch, English.

	Composition.	
Carbonic Acid	12·50	37·8
Azote - -	20·25	59·4
Oxygen - -	1	2·8
	33·75	100·0

Hence we perceive that the irritated torpedo has breathed more than the other. The oxygen absorbed is to the azote absorbed as 100:59; the oxygen absorbed to the carbonic acid produced as 100:37·2. In the second torpedo, the first proportion is as 100:57·50, the second as 100:45. It is a singular result, that the torpedo which has the most action on the oxygen and azote, is at the same time that which develops least carbonic acid. The first result is very easily explained by the acceleration of the respiration and circulation of the irritated torpedo.

I shall yet describe one experiment, which confirms the principle already established, viz., that the activity of the electrical function, is proportional to the activity of the circulation and respiration of the animal. I took a very small and weak male torpedo, whose respiratory motion was, at times, scarcely perceptible, and from which it is very difficult to obtain a discharge. I placed this torpedo under a bell full of oxygen gas. The animal immediately became agitated, opening its mouth several times, making strong contractions, and at the same time giving me five or six strong electrical discharges, after which it died.

To finish the explanation of my researches on the exterior causes, which influence the electrical discharge of the torpedo, I have still to mention the action of poison. I return to the experiments I have already made and published last year. I took three grains of strychnia, and added thereto some drops of muriatic acid. I introduced the muriate into the mouth and stomach of a very lively large torpedo, 25 centimetres wide, and 32 long. In a few seconds it had violent contractions in the spinal marrow: afterwards with these contractions it made some very strong but unfrequent discharges; ten minutes afterwards the discharges became more feeble; but nearer to each other; shortly the discharges ceased, and the animal died with violent contractions. Its life was certainly not prolonged more than from ten to twelve minutes. I also prepared, with three grains of morphine and some drops of muriatic acid, the muriate of morphine. The torpedo I employed in this experiment was still larger than the former one, but not so strong; eight or ten minutes after the introduction of the poison it began giving, by itself without any irritation, and without the least contraction, some extraordinarily powerful discharges; the needle of the galvanometer was in a continual agitation. In ten minutes it gave certainly not less than sixty of these powerful discharges. After this time the spontaneous discharges ceased, and it became necessary to irritate the animal in the mouth and gills to obtain them: it lived thus quietly more than forty minutes, constantly giving more or less powerful discharges.

Among the exterior causes which influence the electrical discharge of the torpedo, we must also place the irritation produced, by compressing it in different parts of the body. Rubbing the gills is one of the most certain ways of obtaining a discharge, as is also the compression of the organ in the point which corresponds to the passage of the nerves. The discharge nearly always takes place, when the fish is bent, so that the stomach becomes concave. Even the compression of the eyes and the cavity. which is placed above the brain, never fails to cause strong electrical discharges. If the nerves which are in this cavity, and which traverse the muscles of the eye, be tied or cut, this compression no longer produces the discharge.

The electric current ought likewise to be placed among the exterior causes, which determine the electrical discharge of the torpedo. A current from thirty pairs, zinc and copper, 5 centimetres wide, charged with a nitro-sulphuric solution, causes strong discharges of the torpedo, every time it is made to pass from the mouth to the gills, to the skin, or in the interior of the organ. I have prolonged the duration of the current, to see what effect was produced when it ceased circulating. I perceived nothing in this case. The exterior application of the current, such as I have described it, either directly or inversely, produces the same effect.

2nd. Section—Interior Causes.

I have already said that by interior causes, I mean those which modify the organization, I shall divide the study of them between three parts of the body of the torpedo.

1*st.* *The proper substance of the organ and the muscular, cartilaginous, &c. parts, which cover and surround it.* I here repeat what I have already said, that in order to study properly these phenomena, I have always taken care to destroy the function of one of the organs: I shall presently show how it may be done.

I have already observed, since the last year, that by raising the skin of the organ, of the back or belly, separately or together, the intensity of the electric discharge is not diminished. I had occasion this year, also, to repeat several experiments of this kind. I cut the organ in the middle, either horizontally or vertically, and introduced a plate of glass to separate the two edges; the electrical discharge still continued. I cut the organ so as to leave one half of it attached to the other by a small strip: the discharge still took place from one to the other, provided that they were connected by a perfect nervous branch. I saw a very lively little male torpedo, of which I cut several times the three quarters of the organ: each time I recommenced cutting, the discharges took place with a constant increasing intensity.

There are only two means by which I was able to destroy the electric function, acting on the single substance of the organ. These two means are, the contact with concentrated mineral acids, and the heat of boiling water. After having raised the upper skin of the organ, I moistened the internal substance with sulphuric acid, and I immediately obtained powerful discharges: in a few minutes, the substance of the organ became white and coagulated, and it was then impossible to obtain any more discharges. The same effect may be produced by muriatic acid. If a torpedo, the skin of one of whose dorsal organs has been raised, be immersed in boiling water, at the first impression of the heat we have very violent discharges; but if we continue this immersion for a few seconds only, the discharge ceases, and the substance is also coagulated. This experiment must be made so that the torpedo be only immersed by the organ that has been flayed, for it is by this means that it is saved. Operating in this manner, I happened to make a curious observation, which I think useful to relate. One of the torpedoes that had lost the electric function of one of its organs, after having been immersed for some seconds in boiling water, was replaced in sea water, where it lived for nearly two hours. The substance of the organ was no longer white or coagulated, having recovered its ordinary properties, without, however, being able to give a discharge.

I add, further, that I cut in two or three points the cartilaginous arc surrounding the organ, the secretory tubes, which reunited in bundles, the cartilaginous arc which is on the gills, and the cavity full of a substance, analogous to that of the organ, which is above the brain, without in the least diminishing the force of the electric discharge. I obtained the same results by cutting all the muscles and tendons which surround the organs.

2*d*. *The nerves which are placed in the organ.* It is a fact already observed for some time past, by Galvani and Spallanzini, that by cutting the nerves of one of the organs, the discharge ceases on that side, while it continues on the opposite side. I had also established in my researches of last year, that it was not sufficient to cut, one, two, or even three, of these nerves to entirely destroy the discharge; but for that purpose it was necessary to cut all four.

(To be continued.)

L. *London Electrical Society.*

Saturday, March 3. Mr. Sturgeon read the third of his series of papers on experimental and theoretical researches in Electricity. This portion is principally devoted to an examination of the assumed identity of Electricity and Magnetism. The most striking instances of analogy are collected, arranged, and illustrated; and the phenomena on which they most obviously disagree distinctly pointed out.

It was our intention to have inserted these papers in this Journal as they were read; but the Society having intimated their intention of publishing them in their transactions, we delay any further notice until they are printed by the Society.

Saturday, March 17. Mr. Leithead, Hon. Sec. to the Society, read a paper describing a series of experiments with single voltaic plates which had been made by the author for the purpose of ascertaining the best solution adapted to the sustaining battery, without the interposition of a membrane. The author had tried, unsuccessfully, solutions of the sesqui-carbonate, and the sulphate of soda; of the sulphate and nitrate of potash; the sulphate of alumina, magnesia, iron, and zinc: also, tartaric and oxalic acids; and the oxolate and nitrate of ammonia; when it occurred to him that, as bodies became materially changed in their properties by chemical combination, a solution of a non-conducting body in an imperfect or non-conducting liquid, might also be altered in regard to its power of conducting electricity. Tincture of iodine was used and the needle was steadily deflected 40 degrees. Next, a solution of iodine in sulphuric ether, but no deflection was produced. Now ether is a definite compound of 4 atoms of carbon, 5 of hydrogen, and 1 of oxygen; and the non-development of an electrical current on the immersion of plates of zinc and copper in a solution of iodine and ether, seems to militate against the opinion of Berzelius, that ether is the oxide of a peculiar compound inflammable body; whilst from the effect with the tincture of iodine it may be inferred, that iodine is an oxide; or, if not an oxide, that in like manner with the acids, it cannot enter into combination with the metals unless oxygen be present, with which they may previously combine. The next solution used was the hydriodate of potash. The first deflection was 65 degrees, but rapidly decreased to 40 degrees; and the action was neither continuous nor steady.

We now arrive at that portion of the experiments which Mr. Leithead considered as most important for the object he had in view. Oxide of zinc is soluble in ammonia; therefore a mixture of nitric acid and hartshorn, the latter in excess, was tried, in the hope that the oxide might be dissolved as rapidly as it was formed; and thus a sustaining battery contrived without the aid of a membrane. The action was energetic but not steady, the deflection being at first 65 degrees. The oxide, however, could easily be shaken off the zinc plate, and by the occasional addition of hartshorn the action of the battery was sustained so as to give a permanent deflection varying from 53 to 65 degrees. Ammoniacal sulphate of copper caused the same deflection; but the objection to its use, is that the zinc plate gradually becomes coated with metallic copper.

A mixture of hartshorn and water alone gave a deflection of 60 to 65 degrees, and it once reached as high as 70 degrees. No perceptible change took place in the zinc plate: that action, however, had commenced upon the copper plate, was obvious from the purple hue which the liquid had began to assume, showing that ammonianeted copper had been produced. From this circumstance, the author was induced to test the direction of the current by the galvanascope; but it was found to be the same as if the battery had been excited by the usual solution. The author thinks there is something singular in this fact; for it has always been understood that the greatest electrical effect is produced, when the most rapid oxidation of one of the metals takes place, while the other metal is not at all, or, *if at all*, very slightly acted on. Mr. Leithead is continuing his experiments: the results will form the subject of future papers.

A letter from Mr. C. V. Walker, to the Honorary Secretary, was also read, the subject of which was the repulsion of two negatively electrified bodies.

Mr. Walker, referring to the work of Dr. Roget, published by the Society for the diffusion of useful knowledge, (article Electricity, sec. 52, &c.,) wherein the supposed action is explained by diagrams, does not consider the arguments to be sustained—that when two pith balls are negatively electrified, it is incorrect to consider them as particles of matter deprived of the fluid, for it is a well known fact that it is only on the surface the action takes place. However, passing over for the present this important fact which may hereafter afford a key to solve this very interesting problem, the author proceeds to trace the consequences which arise from the arguments employed by the talented author of that treatise, to establish the proposition that negatively electrified matter is repulsive.

In conclusion, the author explains what he considers may account for the repulsion of two negatively electrified bodies, on the already acknowledged laws of science. Let 3, 4, fig. 67 be two negatively electrified bodies. The thick lines of the circumferences represent the surfaces deprived of their natural quantity of fluid. The amount of electricity sufficient to saturate each atom of matter is *attracted* to that atom, nor is it partial to one atom more than to another; but since it is repulsive of its *own* particles, it would be more inclined to those atoms in the circumference than to any other, and hence we might expect to find that negative electrified bodies are negative in the centres, and not (as experiment teaches) at their circumferences; but there is a similar action without the circumference that prevents this. Each atom of the circumambient air is surrounded by its electrical *atmosphere*, equally ready to fly to the vacant circumference; and it would actually flow from the *nearest* particle of air, providing the fluid from the *next* particle could supply its place, and so on through the series. But, since non-conducting bodies, very unsteadily admit this transfer, the only effect produced is, that the fluid of that *first* particle flows to the side *nearest* the ball; the fluid of the *second* to the side *nearest* the first; and so on till at some certain distance between the two balls, represented by the line 2, 1, two particles of air X, Y, present to each other negative sides. It will be seen by reference to the figure, where the arrows represent the direction of the forces, that a repulsion exists between the particles immediately within and without the circumferences, marked 1 and 3, 2 and 4; this repulsion reacts on all the particles from 1 to Y, and from 2 to X; so that X and Y are *repulsive*, and therefore *separate*, removing the intermediate particles and the two *balls*, and thus producing the phenomena under consideration.

We cannot close our usual notice of the meetings of the London Electrical Society, without observing that the care displayed in the manner the first transactions have been published, are, in every respect, well worthy of the attention of every friend of electrical science. The transactions to which we allude consist of two papers: the first entitled the action of the voltaic battery shown to be two-fold, and the distinction between the terms quantity and intensity determined by the theory of vibration; with a reply to the various objections made to the theory; by Mr. Thomas Pollock, read 21st of October and 4th of November, 1837. The second, a description of some experiments made with the voltaic battery, by Andrew Crosse, Esq., of Broomfield, near Taunton, for the purpose of producing crystals; in the process of which experiments certain insects constantly appeared. Communicated in a letter dated 27th December, 1837, addressed to the Secretary of the London Electrical Society, read 20th January, 1838.

We have in our former numbers alluded to the contents of these papers. They are now published as part of the transactions of the society, illustrated with a plate of appropriate diagrams. Instead of allowing the different papers to remain until a volume is completed, the committee have considered it more to the interest of the society that they should be distributed to the members as they are printed. We certainly approve of this mode of printing the papers, and the praiseworthy efforts of this society, to further the cause of science which has lately occupied so much of the attention of European philosophers, as well deserving the encouragement it has received.

LI. *Proceedings of the Royal Society.*

1837—1838.

(ABSTRACT.)

December 14, 1837.

JOHN GEORGE CHILDREN, Esq., Vice-President, in the Chair.

The following papers were then read:—

"On the Colours of Mixed Plates." By Sir David Brewster, K. G. H., F. R. S., &c.

In the prosecution of his optical enquiries, the author was induced to study the phenomena of mixed plates, (originally discovered by Dr. Young, and described by him in the Philosophical Transactions for 1802,) as he had observed similar appearances in various mineral bodies, under analogous circumstances, to which he had been led to ascribe an origin different from that assigned by Dr. Young. In order to obtain a more distinct view of these colours, Sir David Brewster employed, instead of the substances used by Dr. Young, the white of an egg, beat up into froth, and pressed into a thin film between plates of glass. From observations of the colours exhibited by plates so prepared, and also by the edge of a thin film of nacrite in contact with copaivi balsam, the author deduces the conclusion, that all these phenomena, as well as those often seen in certain specimens of mica through which titanium is disseminated, and also in sulphate of lime, are cases of diffraction, where the light is obstructed by the edges of very thin transparent plates placed in a medium of different refractive power. If the plate were opake, the fringes produced would be of the same kind as those often noticed, and which are explained on the principle of interference; but owing to the transparency of the plate, fringes are produced within its shadow; and, owing to the thinness of the plate, the light transmitted through it is retarded, and, interfering with the partial waves which pass through the plate, and with those which pass beyond the diffracting edge with undiminished velocity, modify the usual system of fringes in the manner described by the author in the present paper.

December 21, 1837.

FRANCIS BAILY, Esq., Vice-President and Treasurer, in the Chair.

The reading of Mr. Faraday's eleventh series of Experimental Researches in Electricity was resumed, but not concluded.

The Society then adjourned over the Christmas vacation to meet again on the 11th of January next.

January 11, 1838.

JOHN GEORGE CHILDREN, Esq., Vice-President, in the Chair.

The ballot for Bryan Donkin, Esq., was postponed in consequence of the number of Fellows required by the Statutes not being present.

The reading of a paper, entitled "Experimental Researches in Electricity," Eleventh Series, by Michael Faraday, Esq., D.C.L., F.R.S., Fullerian Professor of Chemistry at the Royal Institution, &c., was resumed and concluded.

The object of this paper is to establish two general principles relating to the theory of electricity, which appear to be of great importance: first, that induction is in all cases the result of the actions of contiguous particles; and secondly, that different insulators have different inductive capacities.

The class of phenomena usually arranged under the head of *induction* are reducible to a general fact, the existence of which we may recognise in all electrical phenomena whatsoever; and they involve the operation of a principle having all the characters of a first, essential and fundamental law.

The discovery which he had already made of the law by which electrolytes refuse to yield their elements to a current when in a solid state, though they give them forth freely when liquid, suggested to the author the extension of analogous explanations with regard to inductive action, and the possible reduction of many dissimilar phenomena to one single comprehensive law. As the whole effect upon the electrolyte appeared to be an action of the particles when thrown into a peculiar polarized state, he was led to suspect that common induction itself is in all cases an *action of contiguous particles*, and that electrical action at a distance, which is what is meant by the term *induction*, never occurs except through the intermediate agency of intervening matter. He considered that a test of the correctness of his views might be obtained by tracing the course of inductive action; for if it were found to be exerted in curved lines it would naturally indicate the action of contiguous particles, and would scarcely be compatible with action at a distance. Moreover, if induction be an action of contiguous particles, and likewise the first step in electrolyzation, there seemed reason to expect some particular relation of this action to the different kinds of matter through which it is exerted; that is, something equivalent to a specific electric induction for different bodies; and the existence of such specific powers would be an irrefragable proof of the dependence of induction on the intervening particles. The failure of all attempts to produce an absolute charge of electricity of one species alone, independent of the other, first suggested to the author the notion that induction is the result of actions among the individual and contiguous particles of matter, having both forces developed to an extent exactly equal in each particle.

The author describes various experiments, with the view of showing that no case ever occurs in which an absolute charge of one species of electricity can be given. His first experiments were conducted on a very large scale: an insulated cube, twelve feet in the side, consisting of a wooden frame, with wire net-work, every part of which was brought into good metallic contact by bands of tin foil, had a glass tube, containing a wire in connexion with a large electrical machine, passed through its side, so that about four feet of the tube entered within the cube and two feet remained without; but it was found impossible in any way to charge the air within this apparatus with the least portion of either electricity.

For investigating the question whether induction is an action of contiguous particles, the author employed, as an electrometer, the torsion balance of Coulomb with certain alterations and additions; and for deciding that of specific inductive capacity, a new apparatus, constructed for that express purpose. This apparatus consisted of two hollow brass spheres, of very unequal diameters, the smaller placed within the larger, and concentric with it; the interval between the two being the space through which the induction was to be effected. The apparatus had a tube in the lower part, furnished with a stop-cock, by means of which it might be connected with an air-pump or filled with any required gas. In place of the lower hemispherical shell of air, occupying the interval between the two spheres, any solid dielectric, of the same form, such as shell-lac, glass, or sulphur, might be substituted. Two of these instruments, precisely similar in every respect, were constructed, and the author ascertained that the inductive power was the same in both, by alternately charging each and dividing the charge with the other, and finding that, in all cases, the charge remaining in the one, and also that received by the other, was very nearly half the original charge.

The experiments on which the author principally relies in support of the correctness of his views relative to induction being exerted in curved lines, are the following: a brass ball being laid on the top of an excited cylinder of shell-lac placed vertically, the charge which a carrier ball received when brought to different points near to the brass sphere was measured by means of the electrometer; and it was inferred, from the character of the electricity, that the charge was one by induction, and from its measure, that it proceeded in curved lines. By substituting for the brass sphere a disc of metal above the shell-lac cylinder, it was found that when the carrier ball was brought near to the middle of the disc no charge was communicated, although a sensible one was obtained at the edge of the disc, and also at a point above its centre, farther removed from the excited cylinder. Corresponding and very striking results were obtained when a brass hemisphere was placed on the top of the cylinder of lac. The charge communicated at the centre of the hemisphere was only one-third of that obtained at the edge of its periphery; but by taking it at a point at some height above the centre, and consequently much farther removed from the inducing cause, the charge was nearly equal to that of the periphery. Here, the author remarks, the induction fairly turned a corner, exhibiting both the curved lines or courses of its action, when disturbed from their rectilineal form by the shape, position and condition of the metallic hemisphere; and also a lateral tension, so to speak, of these lines on one another; all depending on induction being an action of the contiguous particles of the dielectric thrown into a state of polarity and tension, and mutually related by their forces in all directions. In the foregoing experiments the dielectric was air; but they were afterwards varied by substituting a fluid, as oil of turpentine, and likewise a few solid dielectrics, namely, shell-lac, sulphur, carbonate and borate of lead, flint-glass, and spermaceti, and with these, corresponding results were obtained. These results, the author considers, cannot but be admitted as arguments against the received theory of induction, and in favour of that which he has put forth.

In the course of these experimental researches, some effects due to conduction, which had not been anticipated, and which were similar to the residual charge in the Leyden jar, had been obtained with such bodies as glass, lac, sulphur, &c. If the inductive apparatus, fitted with a hemispherical cup of shell-lac, after having remained charged for fifteen or twenty minutes, was suddenly and perfectly discharged, and then left to itself, it would gradually recover a very sensible charge; the electricity which thus returned from an apparently latent to a sensible state being always of the same kind as that given by the charge. This return charge is attributed to an actual penetration, by conduction, of the charge to some distance within the dielectric at each of its two surfaces, and several experiments are adduced in support of this view. With shell-lac and spermaceti the return charge was considerable; with glass and sulphur it was much less; but with air, no decided effect of the kind could be obtained. As this was an effect which might interfere with the results, in the method the author adopted for deciding the question of specific inductive capacity, and as time was requisite for this penetration of the charge, its influence on these results was guarded against by allowing, between the successive operations, as little time as possible for this peculiar action to arise.

The author thus states the question of specific inductive capacity which he had proposed to investigate:—Suppose A an electrified plate of metal suspended in the air, and B and C two exactly similar plates, placed parallel to and on each side of A, at equal distances, and uninsulated; A will then induce equally towards B and C. If in this position of the plates, some other dielectric than air, as shell-lac, be introduced between A and C, will the induction between them remain the same; or will the relation of C and B to A be altered by the difference of the dielectrics interposed between them?

The experiment of Coulomb, from which it appeared that a wire surrounded by shell-lac took exactly the same quantity of electricity from a charged body, as the same body took in air, seemed to the author to be no proof of the truth of the assumption, that, under such variation of the circumstances as he had supposed, no change would occur. Entertaining these doubts as to the conclusions deducible from Coulomb's result, he had the apparatus previously described constructed, as being well adapted for this investigation. After rejecting glass, resin, wax, naphtha, oil of turpentine, and other substances, as unfit for the purpose in view, he chose shell-lac as the substance best calculated to serve as an experimental test of the question.

For the purpose of comparing the inductive capacities of shell-lac and air, a hemispherical cup of shell-lac was introduced into the lower hemisphere of one of the inductive apparatus so as to nearly fill the lower half of the space between the two spheres; and their charges were divided in the manner already described; each apparatus being used in turn to receive the first charge, before its division with the other. As the two instruments were known to have equal inductive powers when air was contained in both, any deficiences resulting from the introduction of the shell-lac would show a peculiar action in it, and, if unequivocally referable to a specific inductive influence, would establish the point in question.

The air apparatus being charged, and its disposable charge being 290°, this charge was divided between the two. After the division the charge in the lac apparatus was 113°, and in the air apparatus 114°. From this it appears, that whilst by the division the induction through the air lost 176°, that through lac gained only 113°. Assuming that this difference depends entirely on the greater facility possessed by shell-lac of allowing or causing inductive action through its substance than that possessed by air, then the capacity for electric induction would be inversely as the respective loss and gain; and assuming the capacity of the air apparatus as unity, that of the shell-lac apparatus would be $\frac{176}{113}$ or 1·55.

When the shell-lac apparatus was first charged, and then the charge divided with the air apparatus, it appeared that the lac apparatus, in communicating a charge of 118°, only lost a charge of 86°. This result gives 1·37 as the capacity of the lac apparatus.

Both these results, the author considers, require a correction; the former being in excess, the latter in defect. Applying this correction, they become 1·50 and 1·47. From a mean of these and several similar experiments, it is inferred that the inductive capacity of the apparatus having the hemisphere of lac is to that with air as 1·50 to 1.

As the lac only occupied one half of the apparatus containing it, the other half being filled with air, it would follow from the foregoing result, that the inductive capacity of shell-lac is to that of air as 2 to 1.

From all these experiments and from the constancy of their results the author deems the conclusion irresistible, that shell-lac does exhibit a case of *specific inductive capacity.*

Similar experiments with flint-glass gave its capacity 1·76 times that of air. Using in like manner a hemisphere of sulphur, it appeared that the inductive capacity of that substance was rather above 2·24 times that of air, and the author considers this result with sulphur as one of the most unexceptionable.

With liquids, as oil of turpentine and naphtha, although the results are not inconsistent with the belief, that these liquids have a greater specific inductive capacity than air, yet the author does not consider the proofs as perfectly conclusive.

A most interesting class of substances, in relation to specific inductive capacity, the gases or aeriform bodies, next came under the author's review.

With atmospheric air, and likewise with pure oxygen, change of density was found to occasion no change in the inductive capacity. Nor was any change produced, either by an increase of temperature or by a variation in the hygrometric state.

The details are then given of a very elaborate series of experiments with atmospheric air, oxygen, hydrogen, nitrogen, muriatic acid, carbonic acid, sulphurous acid, sulphuretted hydrogen, and other gases, undertaken with a view of comparing them one with another under a great variety of modifications. Notwithstanding the striking contrasts of all kinds which these gases present, of property, of density, whether simple or compound, anions or cations, of high or low pressure, hot or cold, not the least difference in their capacity to favour or admit electrical induction through them could be perceived. Considering the point established, that in all these gases induction takes place by an action of contiguous particles, this is the more important, and adds one to the many striking relations which hold among bodies having the gaseous form.

In conclusion, the author remarks, that induction appears to be essentially an action of contiguous particles, through the intermediation of which the electric force originating or appearing at a certain place, is propagated to or sustained at a distance, appearing there as a force of the same kind and exactly equal in amount, but opposite in its direction and tendencies. Induction requires no sensible thickness in the conductors which may be used to limit its extent, for an uninsulated leaf of gold may be made very highly positive on one surface, and as highly negative on the other, without the least interference of the two states, as long as the induction continues. But with regard to dielectrics, or insulating media, the results are very different: for their thickness has an immediate and important influence on the degree of induction. As to their quality, though all gases and vapours are alike, whatever be their state, amongst solid bodies, and between them and gases, there are differences which prove the existence of specific inductive capacities.

The author also refers to a transverse force with which the direct inductive force is accompanied. The experimental proof of the existence of such a force, in all cases of induction, is, from its bearing on the phenomena of electro-magnetism and magneto-electricity, of the highest importance; and we cannot but look forward with the greatest interest to the promised communication in which these and other phenomena relating to this subject will be reviewed.

A paper was in part read, entitled "Fourth Letter on Voltaic Combinations." Addressed to Michael Faraday, Esq., D. C. L., F. R. S., by John Frederic Daniell, Esq., F. R. S.

February 1, 1838.

FRANCIS BAILY, Esq., Vice-President and Treasurer, in the Chair.

The reading of a paper, entitled "Fourth Letter on Voltaic Combinations, with reference to the mutual relations of the generating and conducting surfaces;" addressed to Michael Faraday, Esq., D. C. L., F. R. S., &c. By John Frederic Daniell, Esq., F. R. S., Professor of Chemistry in King's College, London, was resumed and concluded.

In this communication the author describes a series of experiments, made for the purpose of determining the distribution of the voltaic force from its source in the generating metal, as indicated by the deposition of reduced copper in the constant battery; and, considering that the voltaic combination most perfect in theory would be one formed by a solid sphere, or point, of the generating metal, surrounded by a hollow sphere of the conducting metal, with an intervening liquid electrolyte, he constructed an apparatus making as near an approximation as possible to these conditions. It consisted of two hollow brass hemispheres, applied to each other by exterior flanges, and rendered water-tight by an intervening collar of leather. In the centre of the hollow sphere thus formed, a ball of amalgamated zinc was suspended by a well-varnished copper wire, connected with one of the cups of a galvanometer, and was contained in a membranous bag holding the acid solution; the whole being introduced through a short tube in the top of the upper hemisphere, and the remaining space being filled with a saturated solution of sulphate of copper. The galvanic circuit was completed by wires establishing connexions between either hemisphere and the other cup of the galvanometer. For measuring the forces developed, sometimes the ordinary magnetic, but in the greater number of instances the calorific galvanometer of De la Rive was employed; the indications given by these instruments were noted, on the completion of the circuit, in various ways; and the deposition of copper in the hemispheres was examined after the apparatus had been in action for a certain number of hours.

The following are the conclusions which the author deduced from a series of experiments thus conducted:

1st. The force emanating from the active zinc centre diffuses itself over every part of the upper hemisphere, from which there is a good conducting passage for its circulation.

2d. The same amount of force is maintained by either hemisphere indifferently; but when both conducting hemispheres are in metallic communication there is no increase of force.

3d. Although the force is not increased, it spreads itself equally over the whole sphere.

4th. When one hemisphere is connected with the zinc centre by a short wire capable of affording circulation to the whole force, and the other hemisphere is connected by a long wire, through the galvanometer, with the same centre, the equal diffusion of the force over the whole sphere is maintained.

5th. There is no greater accumulation of precipitated copper about the point with which the conducting wires are brought into contact, and towards which the force diffused over the whole sphere must converge, than at any other point; proving that the force must diverge from the centre equally through the electrolyte, and can only have drawn towards the conducting wires in the conducting sphere itself. Other experiments showed that the force is but slightly increased by a great increase of the generating surface.

The author's attention was next directed to ascertaining the nature of the law according to which the force emanates from the zinc centre to the surrounding conducting sphere. With this view, a variety of experiments were made with the zinc in different positions in the interior of the sphere; and from these it appeared that, whatever may be its position, the whole force is the same. From these results it is inferred, that the force emanating from the zinc ball diffuses itself over the surrounding conducting sphere in obedience to the well-known law of radiant forces being in the inverse duplicate ratio of the distance.

Experiments of the same kind were likewise made with the previous combination inverted, that is, with a small copper ball in the interior of a large hollow sphere of zinc; and from these the author concludes that, in this case also, the law of radiation is maintained, although the force is reduced to one half of that obtained from the former combination.

In order to ascertain the effect of cutting off the lateral radiation from the zinc ball, it was placed in a glass tube, six inches long, within half an inch of the lower aperture, over which a piece of membrane was tied, and the tube plunged into the solution of copper contained in a brass hemisphere, so as to rest upon the bottom. The results obtained by this arrangement, as also those when the zinc ball was raised in the tube to the surface of the solution, showed that the action of the zinc ball had been propagated from the aperture of the glass tube, as from a centre, diverging from this in the solution.

The experiments next described appear to have an important bearing on a question of vital interest in the theory of electricity, which has been discussed by Mr. Faraday, in a paper recently read to this Society: viz., whether the forces emanating from a centre of electric action act, like other central forces, in straight lines; or whether they are propagated from particle to particle in the surrounding matter, and may, consequently, when obstacles interfere with their rectilinear propagation, exert their influence in curved lines. An elliptical plate of copper, one side of which was covered with lac varnish, was placed in an earthen pan, with the varnished side upwards, and covered to the depth of a few inches with the acid solution of copper. The zinc ball, placed in the tube half an inch from the diaphragm, was plunged just below the surface of the solution, and the circuit being completed, the galvanometer indicated an action nearly equal to that which had been previously observed when both sides of the copper had been exposed. The under side of the copper presented the appearance of a border of precipitated compact pink copper, varying from $1\frac{1}{8}$ to $\frac{7}{8}$ of an inch in width, and the remainder was covered with precipitated copper of a darker red colour, into which the border gradually passed; and similar results were obtained with a circular disc of copper, having one side varnished. It hence appears, that the under surface, which, by itself, is capable of sustaining from the ball in the centre of the solution an action nearly as great as the upper surface, when combined with the latter adds no more than about one-eighth part of its efficiency; and whereas, with the upper surface, the action varies in some inverse ratio of the distance of the generating from the conducting surface, with the under surface, there is a maximum point, on both sides of which it decreases; and this point is doubtless dependent on the angle at which the force which radiates from the ball meets the edge of the plate. The author having been led to the conclusion, that the force developed by voltaic combinations is subject to the law of radiant forces, had been utterly at a loss to understand how, upon this hypothesis, it could extend its influence to the side of a plate opposite to that to which it was directed in right lines; but having perused Mr. Faraday's "Eleventh series of experimental researches in Electricity," all his own results appeared to fall in naturally with the general views therein explained. He considers, that the direction of the force through an electrolyte may be expressed in the very words employed in that paper to describe that of the direct inductive force in staticle electricity, simply substituting the term *Electrolyte* for *Dielectric*, and the term *Current* for *Induction*.

Experiments are further described, in which the effects of various combinations of different generating and conducting surfaces, placed at different distances apart, were measured by the calorific galvanometer, from which the following conclusions are drawn:

1st. That the energy of the force is about sextupled by the absorption of the hydrogen at the conducting surface; except in the case of equal plates, when it is more than quadrupled.

2d. That the effect of distance is much more decided in the instances where the amount of the circulating force is greater, than in the contrary cases.

3d. That the amount of force put into circulation from a large surface of zinc towards a central ball of copper, is, as in former instances of similar combinations, about one half of that from the reverse arrangement.

4th. That a ball of zinc, exposing a surface of 3·14 square inches, placed over the centre of a plate of copper, exposing on its two sides a surface of 28 square inches, sustains an action of nearly the same amount as a plate of zinc, of the same dimensions as the copper, placed at the same distance.

In conclusion, the author remarks, that the principal circumstance which limits the power of an active point within a conducting sphere, in any given electrolyte, is the resistance of that electrolyte, which increases in a certain ratio to its depth or thickness; and this thickness may virtually be considered the same wherever the included point may be placed, but increases with the diameter of the sphere. In an insulated hemisphere, however, the approximation of the active point to the lower surface virtually decreases the thickness of the electrolyte, and consequently the force increases. In this respect, the action of a point upon a plate may be considered the same as upon an indefinitely large hemisphere, towards which, as the point approaches, the force increases.

A paper was also in part read, entitled, "Experimental Researches in Electricity." Twelfth Series. By Michael Faraday, Esq., D.C.L., F.R.S., &c.

LII. MISCELLANEOUS ARTICLES.

Letter from the Rev. N. J. Callan, *Professor of Natural Philosophy at the R. C. College, Maynooth.*

To the Editor of the Annals of Electricity, &c. &c.

Dear Sir,

I find by the January number of the Annals, which I got only on the 9th of February, that you expected, during the last month, a description of the electro-magnetic engine which I am getting made for the College. It was impossible for me to send you a description of the engine, for it is not yet ready for work. The frame to which some of the magnets were fastened, gave way. The engine was made for propelling a carriage. From experiments made with this engine, and with two smaller ones, on a different and better construction, I conclude with certainty that only for the weakness of the frame, and an inaccuracy in the construction of one part, the propelling power of the engine would be equal to that of six or seven horses. But, in consequence of the weakness of the frame to which some of the magnets are fixed, and the want of accuracy in the construction, I am afraid that the power of the engine will not much exceed that of one horse. However, the frame may be still made sufficiently strong, and the inaccuracy to which I allude may still be corrected.

By moving with the hand some of the electro-magnets, sparks are obtained from the wires coiled round them, even when the engine is no way connected with the voltaic battery.

The electric current which produces these sparks is incapable of producing sensible heat, of giving shocks, or of affecting the tongue in the slightest degree. I know not how to account for the production of the sparks, except by supposing that the motion of the iron bars excites in them a certain magnetic power, by which an electric current is produced in the wire coiled round them. When the bars are at rest, they do not exhibit any magnetic power whatever.

I am at present engaged in a series of experiments on the best method of applying electro-magnetism to machinery. The experiments which I have already made, give me every reason to think that I shall be able to construct an engine, which, with a battery containing less than a square foot of zinc, will do the work of one horse. I think it better not to send you an account of these experiments until the series shall be completed.

In the October number of the Annals there is a mistake which I long since intended to correct. In page 478, you say that, for giving the shock, I "employ *long* coils on iron horse shoe magnets." If you look to the number of the Annals for May, page 300, you will find that I recommend that the length of the iron bar, and consequently of the coil, should be proportioned to the number and size of the plates in the battery to be employed in magnetizing the bar. Hence, for a small battery, I recommend a short bar of iron and a short coil. In page 299, I state that on straight bars, the wire can be coiled at right angles to the axis of the bar with greater facility than on bars of any other form; and in page 297, I state that the highest intensity is obtained by coiling the wire at right angles to the axis of the magnet. Indeed, the magnet described in page 301, line 12, of the number for May was a straight one. It was made in the preceding January, and was sent to Downside College in the month of May. I believe I was the first who used two coils for the shock, and that the electro-magnet which I had the honour of presenting to you, and which you were kind enough to accept and to show to the Electrical Society, was the first magnet of the kind ever exhibited in London.

I have the honour to remain,
Dear Sir,
Your very obedient and humble Servant,
N. J. CALLAN.

Maynooth College,
February 20th, 1838.

Physical character of Conicine, the active principle of Hemlock. (Conium Maculatum).

By Boutron Charlard and O. Henry.

These Chemists appear to have been very successful in obtaining large quantities of *Conicine* (a term which they adopt from Berzelius, in preference to all others). The object of their researches was to settle the question whether the objection of Deschamps to the conclusion of Geiger (viz. that the Conicine of Geiger owes its alkalinity to Ammonia, and that if the active principle of Hemlock is an alkaloid it still remains to be isolated) is well founded or not. We need not here detail the process by which they procured the narcotic principle, nor the precaution used to ascertain its freedom from Ammonia. The conclusions they arrive at are:

1. That there exists in Hemlock, (*conium maculatum* of L.) and in the seeds of that plant, a peculiar volatile principle which is very poisonous.

2. That this principle, when pure and exempt from water, is in the form of a liquid of an oily appearance, lighter than water, and possessing in a high degree the property of saturating acids and forming crystallizable salts.

3. That contrary to the opinion put forth by some chemists, this alkalinity is inherent, and is not owing to the presence of ammonia.

4. That this principle, which is the first example of a liquid, volatile, vegetable alkali, and which has been designated by the several names of *Conincientine*, *Coneine*, and finally *Conicine*,* ought now to be ranked among organic salifiable bases.

In reply to a subsequent objection of Deschamps that its alkalinity is owing to the caustic potash or soda used in its preparation, they assert that it may be obtained by calcined magnesia, and even by neutral acetate of lead.

It appears also that *Conicine* has been analysed by Liebig, and found to consist of

Carbon . .	66.91
Hydrogen . .	12.00
Azote . .	12.80
Oxygen . .	8.29
	100.

Annales de Chemie, Avril, 1836.

Franklin Journal. G.

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* We prefer the more analogical term proposed by Dr. Wood in the U. S. Dispensatory—*Conia.*—Trans.

On the latent heat of Carbonic Acid Gas. By M. BISCHOFF.

In decomposing Carbonate of Lime by heat in a gun barrel, I found the end of the barrel at 144⅓ Far. while the current of gas indicated only 83¼ F. If we suppose the temperature to which the Carbonate of Lime was exposed to be equal to the melting point of gold, or about 2500° or 2600° F. the gas would have rendered latent 2430 to 2500° Far.

When chalk is decomposed by weak sulphuric acid, the mixture marks 122° F. and the gaseous current 86°. When the acid is concentrated, the mixture is 212° F. and the current 133°.

Ann. de Pog. in Ann. des Mines.
G.

Franklin Journal.

On the use of Ether in Analyses. By M. DOBEREINER.

If to a solution of chloride of manganese and cobalt in alcohol, fifteen to twenty times its volume of ether be added, all the chloride of manganese will be precipitated and the liquid become of a beautiful blue. In adding water, the chloride of cobalt is precipitated of a rose colour; and by re-dissolving in alcohol and employing ether again we may separate the whole of the chloride of manganese.

Ether has a strong tendency to form a special combination with twice its volume of alcohol, and this tendency may frequently be turned to profit in chemical analysis.

An alcoholic solution of hydrate of potash, which contains ·25 to ·30 of water is entirely decomposed by ether, so that the potash separates in the state of an aqueous solution; but if the alcoholic liquor contains no water, the ether does not separate the potash, and the separation is effected only by adding water to the mixture.

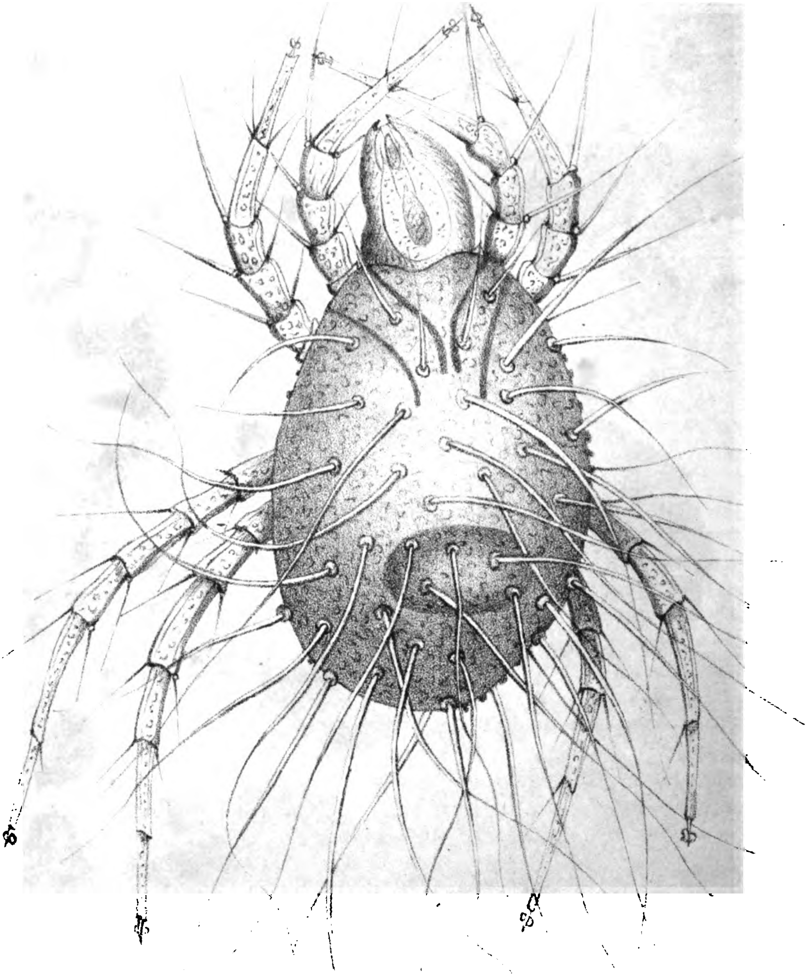
If a mixture of nitrate of lime and nitrate of strontian be treated with alcohol, and that independently of the first salt a little of the second be dissolved, the whole of the latter may be precipitated by adding ether and allowing the liquid to remain some time at rest.

Ann. des Ph. in Ann. des Mines 1836.
G.

Franklin Journal.

PLATE. X.

ACARUS.



Microscopic View of a Female containing an Egg. The Insect is a Specimen of those which appear in Mr Crosse's Galvanic Experiments.

On Stone by Robt Page
W. Annan. lith. 12. Gracechurch St

PLATE. IX.

Fig. 69.

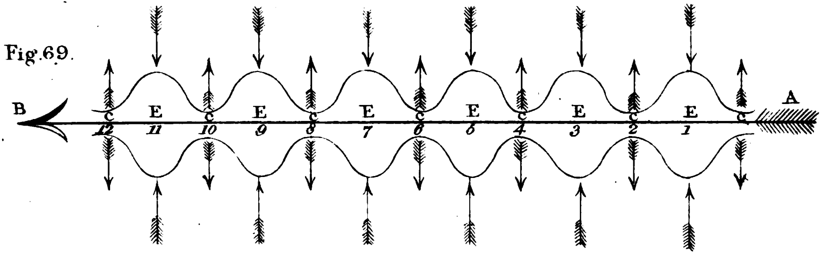


Fig. 70.

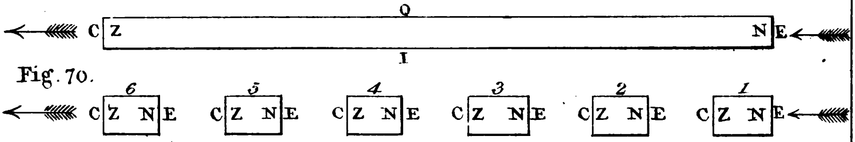


Fig. 68

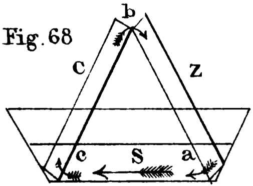


Fig. 71.

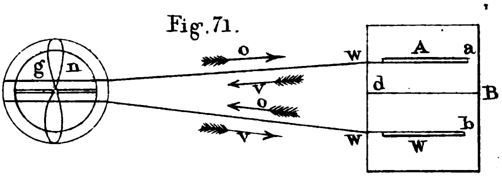


Fig. 72.

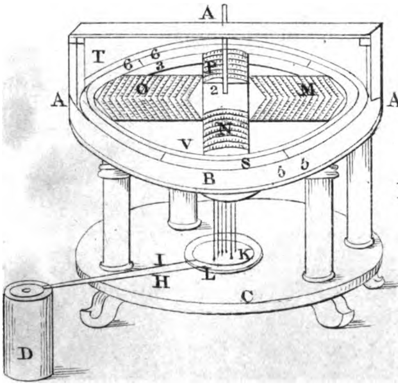


Fig. 74.

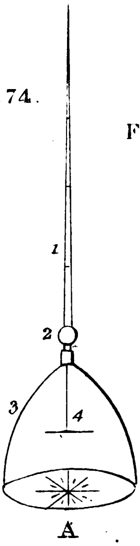


Fig 75.

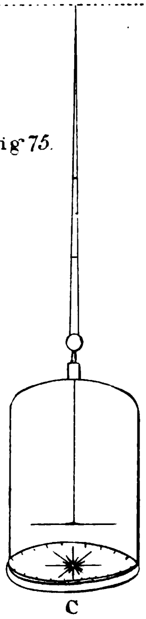
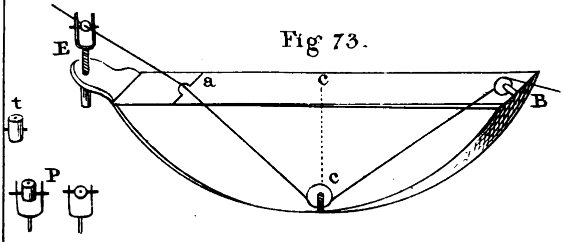


Fig 73.



W. Annan lith. 12 Gracechurch St.

THE ANNALS
OF
ELECTRICITY, MAGNETISM,
AND CHEMISTRY;
AND
Guardian of Experimental Science.

MAY, 1838.

LIII. *Physical, Chemical, and Physiological Researches on the Torpedo. By* M. CHARLES MATTEUCCI.

(Continued from page 302.*)*

I have this year observed, that the display of the torpedo, when two or three of these organic nerves have been cut, is limited to the points in which the ramifications of the untouched nerve are to be found. When care is taken to wipe the skin of the torpedo perfectly dry, this limitation of the discharge may be very well perceived with the galvanometer.

The torpedo may live for a long time, even after the nerves of the organ have been cut. For example, I cut three nerves of the right organ of a very small and lively torpedo: after the operation, the skin was reunited and sewed; and the fish, tied by the tail, placed in the Cesanatican canal, on the 27th. July, at 3, P. M. and died on the evening of the 28th, after having lived about 30 hours. The change caused to the substance of the organ was great in the part where the three cut nerves ramified; it was so minced and consumed in that part, that it was impossible to recognise it; the substance of the nervous trunks had become pulpated; the remainder of the organ was untouched.

It is not necessary to cut the nerves to destroy the electric discharge; tying them is quite sufficient, which with a little practice may be done very easily. The same phenomenon experienced by cutting the nerves will take place if they only be tied.

When the nerves have been cut, and thus all the electric function destroyed, if we draw with a tweezer one of these nervous trunks, which are attached to the organ, we still obtain some electric discharges. It is necessary, in order to insure the success of this experiment, to have a very lively torpedo; in this case the phenomenon never fails to take place.

By moistening, with a very concentrated solution of potassa the nervous trunks of the uncovered organ, the discharge disappears apparently without the nervous substance being altered.

3d. Finally, *the brain*. With the blade of a razor a little sharpened the brain of the torpedo is soon discovered. If the animal be still living, the following may be observed: every time we touch with a pen, a tweezer, a glass tube, &c., the brain of the torpedo, the electric discharge takes place; we have no difficulty in perceiving which are the real points of that organ, whose irritation produces the discharge. It is better for this study that the torpedo be a little enfeebled. The first lobes (cerebral) may be irritated, cut, or entirely destroyed, without the discharge ceasing to take place. The lobes which follow the first, when touched or hurt, undergo strong muscular contractions, and, sometimes, if the animal be very lively, even electrical discharges; however they may be cut without preventing the discharge. The third lobe may be irritated, hurt, or entirely removed, without contraction, and without even then stopping the electric discharge.

The last lobe of the brain, which I look upon as a swelling of the elongated marrow, from whence the nerves which go to the organ proceed, is the only part of the brain that cannot be touched without having very powerful electric discharges. This being destroyed, all electrical discharges cease, even though the rest of the brain be left untouched. On another torpedo I cut the elongated marrow, at the point whence it issues from the brain, that is to say, after it has given the nerves to the organs. Violent discharges and muscular contractions take place when this operation is made, but the electric discharge still continues when we touch the last lobe, which I shall henceforth call the *electrical lobe*. The electric discharge maintains a great force, even when we have cut a large nervous bundle formed by the first nerves of the spinal marrow, and which, divided into two branches, surround the organ, passing above and below the cartilagenous arc.

The organs of the electric function are thus reduced to the last lobe of the brain, its nerves, and this organ. The action of this last lobe on the electric function is *direct*. Hence it is that if we touch the right side of the electric lobe, it is the right organ that gives the discharge, and vice versâ if we touch the left side.

I now pass on to the experiments I made on a dead torpedo. I call the torpedo dead when its gills no longer move, and when irritated, wounded, and compressed, exteriorly and interiorly, except certain points of the brain, it no longer gives electrical discharges. I shall remark, in this place, that the torpedo is not quite dead, at least according to the preceding definition, even when its large blood vessels are cut, and the circulation thus destroyed; in the latter case we still obtain some electrical discharges by irritating the animal. Let us take a dead torpedo, as I have called it, and expose the brain. The first experiment that I shall relate has been known since my work of last year. If we touch the electrical lobe, the discharges appear, and much stronger than those the animal gives while living; the other parts of the brain, though irritated, give no discharge. The action of the electrical lobe is *direct*, and the current of the discharge is driven as usual, from the back to the belly. A certain time having elapsed, I caused the discharges to cease, simply by touching the electrical lobe, but the discharges reappear if this lobe be hurt. What is still more extraordinary, is, that the discharges I obtained by injuring the electrical lobe are *indifferently* driven from the back to the belly or from the belly to the back. I have observed several, one after the other, directed in this latter way. I have also observed these facts this year on a great number of torpedoes. The discharges I obtain by the injury of the electrical lobe are only four or five in number; after that all electrical phenomena is for ever destroyed. I am therefore right in concluding that the direction of the discharge of the torpedo depends on the brain.

There now remains for me to explain what is the action of the applied electric current on the brain and on the nerves of the organ of the torpedo. It is this part that I look upon as the most important of these researches. The pile that I employed was a column, of which the zinc and copper pairs had a surface of 4 centimetres. The liquid of the pile was sea water and 1-10th nitro-sulphuric acid. I have always employed a pile of twenty pairs.

I exposed the brain of a large torpedo, which, although enfeebled, was still living. I introduced the negative réophore of platinum in the organ, on the dorsal side, and near the exterior edge. The torpedo was covered with prepared frogs, and two galvanometers were placed as usual on the two organs. I commenced by lightly touching with a tweezer the electrical lobe. I obtained several discharges, but in a few seconds they ceased, even by touching it. Then I placed the positive reophore on the right side of the electrical lobe, that is to say, the same side as the negative reophore; immediately there was a discharge from the organ. I think it important to assure the reader that this discharge, demonstrated by the convulsions of the frogs and by the galvanometer, is not due to a portion of the current of the pile, which traverses the frogs and the galvanometer. In fact, I acquired by other experiments the certainty that the same current which is made to traverse other parts of the body of the torpedo, besides the organ, and under the same conditions, gives no signs either to the frogs or to the galvanometer. I cut a torpedo in the middle of its body, so that no part of the electric organs remained attached to the lower side. The galvanometer and the prepared frogs were placed on this latter part of the body of the torpedo, the current of the same pile passed from the spinal marrow to the muscles of the tail, without exciting any contraction in the frogs, or giving any sign to the galvanometer. This half of the torpedo was, on the contrary, violently agitated at every passage of the current. I shall now return to the first experiment. If, instead of touching the right side of the electrical lobe with the positive pole, we touch the left, it is the left organ which is discharged, and hence a new proof that these discharges are in reality from the torpedo. The frogs and galvanometer of the left organ are not even included in the circuit of the pile. If the positive reophore touch the whole of the electrical lobe, the two organs are discharged at once. We will now change the direction of the current, that is to say, let the positive pole be placed in the organ, and the negative touch the electrical lobe; we then have strong muscular contractions and *no discharge* of the organs; the galvanometer and the frogs do not move, which is another proof that the discharges obtained above, are really belonging to the torpedo. I again renewed the direct action of the electric current, and although the animal was much enfeebled, the same phenomena were reproduced, viz. there was a discharge of the organ at every passage of the electric current. We must particularly observe that when the torpedo is endowed with a great vitality, the discharges are also observed for a certain time, when the current is inverse, that is to say, that it goes from the organ to the brain.

I wished to consider also what was the effect of tying the nerves of the organ. In this experiment I tied the four nerves of the right organ of a large and very lively torpedo. I exposed the brain and repeated the preceding experiment. When the current passed directly, there was no discharge of the organ; when in an inverse direction I observed only very slight contractions, and hence even another proof of the real nature of the discharges of which I have spoken. I repeated these experiments on fifteen individuals, always with the same result, leaving the nerves untouched, sometimes cutting or tying them, and always taking care to commence the passage of the current, after being certain that the contact of the platina reophore, except it were attached to the pile, submitted to no discharge of the organ. It is proper to observe, that these discharges produced by the current, have not the force of those the animal gives while living; but they certainly do not differ from the last discharges we obtain from the dead torpedo, by lightly touching its electrical lobe. The deviations of the galvanometer are in this case as in the other, from five to six degrees; but they are sufficient to show clearly the deviation in its ordinary direction, viz., from the back to the belly. I shall also observe that we never have the indication of the discharge of the organ, by touching with the positive pole the muscles, skin, liquid of the brain, &c., all points which do not differ from the electrical lobe by their position and conductibility, which is a still further proof of the real nature of the preceding discharges.

The action of the electric current on the nerves of the organ is also important, and ought to be described with the greatest care. I separated one of the organs of a torpedo which was still living; it was a very large female torpedo, the largest of the 116 that I had; it weighed 6 pounds (3 kil). The organ was separated without detaching the skin; I only cut the nerves and gills, cutting circularly all the parts which surround the organ at the side of the head. There thus remained the organ with its four nerves, which, was a little drawn out, to about 2 or 3 centimetres; all of which were placed on a plate of glass. Then after having placed the galvanometer and the frogs on the organ as usual, I introduced the negative réophore into the substance of the organ, near the exterior edge, and with the positive réophore touched one of the four nerves which were extended on the glass plate. There was immediately a deviation of four degrees in the galvanometer in the ordinary direction of the current of the torpedo, and strong contractions in the frogs; by touching the other nerves the same phenomena took place. I touched the substance of the organ which is between the nerves, and that in several points, such as the skin or several attached muscles, but no phenomenon resulted. I reunited the four nerves on a platina plate, and it was by touching this plate that the preceding phenomena indicating the discharge of the organ, were reproduced with the greatest intensity. I also cut the ramification of one of the nerves, with the substance in the interior of the organ, leaving the exterior nervous trunk untouched; if this trunk be touched by the positive pole, the indications of the discharge fail. I tied the nerves, and the discharges failed also when the current passed. Repeating these experiments several times, and on several individuals, I sometimes saw the phenomenon of the discharges, when touching the substance of the organ with the positive pole: but a slight attention showed me, each time, that the pole was in contact with some of the nervous threads spreading over the organ. The difference between the action of the electric current on the nerves alone, and its action on the brain connected by the nerves to the organ, ought to be remarked; we have seen that in the second case the inverse current produced no discharge. The contrary happens when the nerves and the substance of the organ alone are traversed by the electric current. There is a discharge of the organ when the current goes from the nerves to the organ, and also a discharge when the direction of the current is contrary. The galvanometer always deviates in the same direction, and this proves still better that it is the discharge belonging to the torpedo which produces it. If the torpedoes had been dead for some little time, the action of the electric current that we have described, on the nerves and the organ, and on the brain connected with the organ, would be entirely destroyed, and it is vain to try to reproduce it by a greater number of pairs. This result, happening after a certain time, and depending on the degree of vitality of the animal, and the variable treatment to which it is submitted, may, if necessary, serve also to prove the exactness of my assertion.

I thought it also important to determine the conducting power for the electricity of the nervous substance and of that of the organ. I have done that with the exactness that it is possible to effect in this kind of experiment. I employed a double galvanometer, and made the two currents pass by a strip of the substance of the organ, and by five or six nervous trunks of the reunited torpedo. I used a pile of 20 pairs. The conductibility seemed to me always stronger for the substance of the organ, and that seems to me very easy to conceive.

Conclusions.

When we reflect, 1st, on the facts that have already been established in our first work on the torpedo, viz. that no track of electricity is found in the organ, except when it discharges itself. 2d. that we may destroy the skin, muscles, cartilagenous arc which surrounds the organ, and a great part even of the substance of the organ, without the discharge ceasing, or even being enfeebled. 3d. that narcotic poisons give strong electrical discharges. 4th. that irritation of the electrical lobe of the brain, after death, gives very strong electrical discharges. 5th. that by drawing and compressing the nerves only, we have a discharge. 6th. that strong muscular contractions are observed in the parts which surround the organ, with the discharge taking place. 7th. that injuring the electrical lobe of the brain causes discharges whose direction is no longer constant from the back to the belly, but sometimes go from the belly to the back. 8th. finally, from the last facts that I have related on the action of the electric current, it is impossible not to draw the following conclusions:

1st. The element necessary for the electrical discharge of the torpedo, and for the direction of this discharge is produced by the last lobe of the brain, and transmitted by the nerves in the substance of the organ.

2d. Whence results, that it is not in and by the organ that this element is prepared.

3d. An electric current, directed from the brain to the organ by the nerves, causes the discharge as well as this element would do it, which I think may be regarded as electric fluid.

4th. Since the electric discharges of the torpedo, even under the influence of the electric current, cease when the nerves are tied, we must admit that this element, which I look upon as analagous to the electric current, and like the electric current itself, requires to give it effect, a molecular disposition in the nerves, the destruction of which causes the cessation of the function.*

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* M. Becquerel's hypothesis for explaining the muscular contractions seems to me to enter into the explanation that I had previously given of the shock which the frogs experience when the inverse current ceases to traverse them. These phenomena may be thus understood: the *direct* current displaces the nervous globules in the direction of the current, and in this case we have a contraction. When the current ceases, the globules return to their place, but the motion does not occasion any contraction: on the contrary, it corresponds to what we call *sensation*. It is now clear that when the current is *inverse*, we have no contraction by its introduction, because the displacement of the globules, which always happens in the direction of the current, is in this case the same which is produced by the direct current, which ceases to pass. Hence we see, that when the *inverse* current ceases, the globules in returning to their place, have the same motion as the same globules when disturbed by the direct current; we then have, as in this case, a contraction.

CHAPTER V.

On the Electricity of the Torpedo, and all animals generally.

I think the function of the torpedo is now better known. It is an animal having a special organization, by means of which the electric current may be modified, so as to be changed by the charge of a battery or pile. We are ignorant of what the organization necessary to produce this effect is. Undoubtedly the apparatus for the condensation of the electric fluid, which exists in the organ of the torpedo, is not like those with which we are acquainted. Here remains a great discovery to be made for philosophy, and which may even be made without this fish. Two conditions are necessary for this organic function; 1st, that the albuminous substance, which composes a great part ot it, be not coagulated, although this coagulation may take place without destroying the electric conductibility of this substance: 2d, that the nerves which enter into the organ have their perfect organization. If the nerves be tied, the electric current passes equally; but there is no discharge. Hence, we have another function in the nerves, besides that of transporting the electric current; and this other function requires that perfect normal organization, which we have yet to discover.

Having thus decided the electric function of the torpedo, there now remains only to resolve a problem of general physiology. Is the electricity prepared in the animals? Are the brain and the nerves more fit than other parts of the animals to prepare and conduct this electric fluid? If it be so, what is the physico-chemical action, to which we may compare this production of electricity in the animals?

We are indebted to Galvani for a great fact: viz., the thighs of a recently prepared frog, bent on the sciatic nerve, contract as if by the effect of the passage of an electric current. It has been wished in these latter times to perceive in this fact a case of electricity developed by the chemical action of different animal liquids, or even a thermo-electric current. A repetition of this experiment, after having washed the prepared frog three or four times in distilled water, is sufficient to cause the explanation to be rejected. The contractions, although more feeble, still take place when the nerve and muscles are placed in contact. The celebrated Humboldt observed these contractions, when the nerves and muscles were placed in contact by a piece of muscular substance. Some experiments of this kind may be found described in Aldini's treatise on galvanism. When we touch with the spinal marrow of a prepared frog, any part of the brain, muscles, or opened bowels of an animal, either still living or just killed, we always observe strong contractions in the frog. M. Nobili, with his very sensible galvanometer, has obtained by the actual current of a frog, a tolerably good deviation; and certainly the different parts of a frog, that has been a long time dead, and moistened in saline acid solutions, and alkalies, at different degrees of temperature, never give so sensible and strong a current as that of the frog. I have frequently seen my galvanometer, which is rather sensible, indicate the current of the frog; but it has never happened with the above mentioned solutions.

I tried to reproduce these same experiments on the torpedo. Every time a recently prepared frog touched, with its nerves, the brain of the torpedo, it was strongly contracted, and these contractions were still stronger when a drop of blood was poured on the points touched. I have even seen constantly the real contractions of the frog, revive powerfully by the effect of a drop of fresh blood of the same animal, poured among the muscles and nerves in contact. I have varied and repeated, in all ways, these experiments, and I have been led to conclude that, every time blood, or liquid, whether organized in muscular substance, touch the nervous substance organized in the nerves, in the elongated marrow, or in the brain, there is a production of an electric current. This current remains a certain time after death, requiring for its production a certain degree of vitality, and is constantly directed from the sanguine or muscular molecule to the nervous. The fine observation of M. Donné, on the electric currents that he has discovered among the organs of secretion, will terminate by entering into the phenomena above mentioned. Although the facts that I have related, may suffice to demonstrate, that the origin of this current is neither thermo-electrical nor electro-chemical, I have always conceived that a deeper study of the actual current of the frog, would perhaps be of some importance.

I first discovered that we could very well observe the actual current in the living frog. Cutting longitudinally the skin of its sides, and drawing out one of its spinal nerves, with a tweezer or wooden point. Raising the skin of the thighs, and placing the thigh on this nerve, contractions are exhibited at each contact. The thighs may be perceived without raising the skin, and by this means the animal is preserved for some time. This experiment, like that of Galvani, does not succeed on all frogs. I wished to consider the action of heat on this peculiar current, which action is very important. As soon as a piece of ice has covered a frog, four or five minutes, this peculiar current is destroyed, the animal being still living. Refreshing the frog again, by blowing oxygen into its lungs, I have sometimes succeeded in strongly exciting the animal, and then the peculiar current has reappeared. In most cases, however, when the action of the cold is prolonged, the animal lives, but the peculiar current fails. This analogy, or rather this identity, of the action of heat on the electric function of the torpedo, and the peculiar current of the frog, seems to me a demonstration of the existence of force common to these two phenomena. The first fact that I remarked, in observing this peculiar current on the living animal, was that it is more feeble than the current we have after its death, and however lively the frog may be, it becomes weaker after a certain time, and terminates by its final disappearance.

We must wait until this current has disappeared by itself, to witness the production of a very singular phenomenon. Let the frog be then cut and prepared after the manner of Galvani: a violent contraction will be seen by putting the thigh and nerves in contact nearly in the same point as it was made, the animal being still living. I then observed that if we wait a certain time, we see these contractions also disappear; but they may be reproduced by cutting the spinal nerves at their origin, or where they issue from the spinal marrow, and touching them again with the thigh.

These facts have no connexion with the physiological law established by Ritter in his time, viz., that the sensibility of the nerves, continues diminishing from its origin to their ramifications. In my mode of operating, they are the same points of the nerves and muscles which are touched. The following is the fact that may be deduced from the law of Ritter; when the spinal nerve ceases to give peculiar currents, let its prolongation which is hidden in the muscles of the thigh be discovered; if the muscles be touched with this part, we shall again have violent contractions. This case differs from that of Ritter, the peculiar current being the cause of the contraction.

I now return to the distinct characters which distinguish the peculiar current of the frog, from a thermo-electrical or electro-chemical current. In the first place, the direction of the current is quite opposite to what it would have been, if it had a chemical origin, or, at least, we must have supposed the muscles charged with alkali, and the nerves with acid, which is contrary to all we know of their chemical composition. I discovered afterwards two extremely decided differences. I compared the peculiar current of the frog, with a current developed by the contact of a solution of nitric acid, and one of potash. When I had proved the existence of the contraction, by placing the muscles and nerves in contact, and causing the current of electro-chemical origin to pass, I tied the spinal or crural nerve with a thread in the middle of its length; I then rebent the thigh above the ligature; there was no longer any contraction. I touched it beneath, and it remained as before. I then caused the electro-chemical current to pass, and found that a contraction was excited, whether it passed above or below the ligature. Another difference which is not less decisive is, that, whilst the peculiar current is prolonged, even for half an hour, the electric current on the contrary, produced by the two acid and alkaline solutions (nearly 1-40th acid and alkali) no longer excites contractions.

I shall add finally, that binding the nerve in no way destroys its conductibility. In fact, I have made the current of a single pair pass by the two spinal nervous threads of a frog and by a galvanometer in the same time. I waited, to tie the nerve, until the needle was fixed; at the moment of the operation, I observed a slight movement in it, which is sometimes more and sometimes less, after which it stops as before. Hence this motion is not due to a diminution of conductibility in the nerve, produced by the ligature, nor to a greater intensity of the current caused by the chemical action of the two solutions; since this latter current ceases to cause the frog to contract, before the peculiar current. The following is what may be concluded from these researches on the peculiar current of the frog.

1st. The peculiar current of the frog has the same origin as that produced in the brain of the torpedo and which charges the organ.

2nd. This current cannot develop itself and excite contractions, or *functionate* in general by the nerves, without the organization of the nerve itself be untouched, in all its successive ramifications.

It also appears to me that we can tolerably well understand the facts established on the peculiar current, where the nervous circuit comprising the brain, marrow, and nerves, is complete; the electric fluid should circulate in it in a complete manner, and there is no reason why we should separate a part of it. It is only when the animal is over excited that we can prove its presence. We conceive from this how the peculiar current disappears on the living animal. But if this circuit be destroyed, which is the case when the frog is killed and prepared after the manner of Galvani, the electricity may then change its course: this peculiar current is effectively perceived to be stronger on the dead frog, and we frequently have it on the dead frog, when we cannot observe it on the living animal.

Hence we have no longer any difficulty in conceiving why we have not yet succeeded in having indications of a current in the nerves.

I hope it will not be concluded, from this, that I admit of unknown vital forces. Such an idea is far from my thoughts; I have only seen in the organic functions, the effects of grand physical forces of general agents, acting upon this *mysterious* molecular dispositions which are called organization. I am much pleased, in the interest of the science, to see one of the greatest philosophers of our time, carry on in this direction his researches and important philosophical labours.

As to the torpedo, the problem of its electrical function seems to me more clearly settled now than it was. There are in the torpedo, as in all animals, physical and chemical (vital?) reactions, which develop electrical currents; it has a special organ, in which the electric current introduced by the nerves, is condensed and gives place to the electric discharge proper to this fish.

CHAPTER VI.

Chemical analysis of the substance of the organ.

I analysed the substance of the organ of a middling sized torpedo, after having stripped it of all the membranes, muscles, and large nervous trunks which are attached to it. I began by determining the quantity of water it contained, and proceeded by the usual method. In one experiment I obtained from 1120 parts of substance 104 of dried produce: in another experiment from 1307, 136 dried parts. The mean quantity of water is thus reduced to 903·4 on every 1000 of the substance of the organ. The analyses of the dried produce was made by putting alcohol at 36° with it and renewing this solution three times, at intervals of 24 hours. I renewed the residue by the same boiling alcohol and repeated this treatment twice. Finally the rest was treated by boiling water and afterwards by concentrated acetic acid. The following is the result: gr 6·65 of the dried produce gave me

gr. 3·171 substance dissolved in cold alcohol (A).
0·893 .... boiling water (B).
2·587 substances insoluble in alcohol (C).

The products A and B are composed of muriate of soda, lactate of potassium, lactic acid, extract of Berzelian meat, a fat substance analogous to the eläine of the brain, and of a fat substance, solid at the ordinary temperature. The produce C is almost entirely composed of albumine, and some traces of gelatine.

When the alcoholic solution obtained by cold is evaporated, it first forms crystalline beds, then drops of yellowish oil: the latter are deposited at the bottom of the liquid. This liquid is extremely acid, and forms a precipitate with an infusion of gall-nut. When all the solution is evaporated, there remains a yellow greenish, oily, very acid and deliquescent mass. It is almost entirely dissolved in water, making a kind of emulsion. It disengages a smell of rancid fish oil. Potassa dissolves the fat substance, destroys the odour, and neutralizes the liquid: tartaric acid added re-establishes the fat acid, and gives by evaporation and distillation, lactic and phocenic acid. The produce of boiling alcohol gives also lactic acid, and a fat solid substance, which, mixed with nitric acid, shows traces of sulphur and phosphorus. The substance insoluble in alcohol, boiled in distilled water, gives a solution of a white salt which is troubled by the bichlorure of mercury: the infusion of gall-nuts gives it a flaky precipitate which is partly dissolved by heating the liquid: the remainder is soluble, especially by heat, in acids and acid alkaline solutions. It is only pure albumine.*

The albuminous substance which covers the brain, only differs from the substance of the organ by a greater quantity of water.

It would be impossible for me not to remark the analogy which exists between the composition of the cerebral matter, and that of the electric organ of the torpedo, which we have just analysed.

LIV. *The Action of the Voltaic Battery shown to be two-fold, and the distinction between the terms Quantity and Intensity determined by the theory of vibration; with a reply to the various objections made to the theory.* By MR. THOMAS POLLOCK.†

Read 21st of October, and 4th of November, 1837.

As the subject of this paper involves a general and important law, and may have a considerable influence in simplifying the theory of voltaic action, the investigations I am about to present to the notice of the Society will not, it is hoped, be unworthy of consideration. If it can be shown that the action of the battery is twofold; first in abstracting the electric fluid from bodies, and secondly in imparting it to others, an important point will be gained, and the progress of electrical science, in a steady course, amidst the conflicting opinions of the present day, may be anticipated. When we are informed, although on the very highest authority, that an atom of water yields as much electricity during its decomposition as is contained in a flash of lightning, we may well be startled with the assertion; but when we know that the power decomposing the atom of water arises from the abstraction of the electric fluid, and that the flash is an effect of the communication of the fluid, our astonishment ceases.

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* When the dried substance of the organ is mixed three times with cold ether and the solution evaporated, we obtain a greasy yellowish matter, of the appearance of mother of pearl, which is slightly dissolved in ether and cold alcohol: it is without taste, of an insipid smell and saponifies by potassa: burnt and calcined in a platina crucible it leaves an acid cinder, and, mixed with boiling nitric acid, shows traces of sulphuric and phosphoric acid. Hence it is the cerebral stearine.

† From the Transactions of the London Electrical Society.

This subject resolves itself into four parts.—I. The alteration of form which the elements of the battery undergo. II. The explanation of these changes on the theory of vibrations. III. The distinction between the terms quantity and intensity determined by the theory of vibration. IV. The various objections made to the foregoing theory.

I. ON THE ALTERATION OF FORM WHICH THE ELEMENTS OF THE BATTERY UNDERGO.

No attempt appears to have been yet made to exhibit the connexion that exists between the alteration of form in the elements of the voltaic battery, and its electrical properties. As far, however, as my experiments have led me, there is every reason to believe that the electrical action of the battery depends on these changes.

That we may present a distinct view of this curious investigation, it will, perhaps, be advisable to consider the phenomena of the pile in the order they may be supposed to follow. This may be done by a few remarks on *the transition of zinc from the state of a metal to that of an oxide; the transition of the oxygen from the state in which it exists in water to that in the oxide of zinc; and the change effected by the union of the acid in the solution with the oxide of zinc.* To pursue this subject further, it might be advisable to trace the influence of these changes of form on the remaining phenomena of the pile, such as the *influence of the formation of the oxide upon the zinc; the influence of the zinc upon the copper, the copper upon the hydrogen*, and lastly, the *action of these upon the decomposing power of the pile.*

On the transition of the zinc from the metallic state int that of an oxide. This effect, there is every reason to believe is the primary cause of voltaic action in the battery. The most remarkable circumstance attending this change of state is an increase of volume; the density of the oxide of zinc being about 3 and that of the metal about 7. Now the space occupied by any body must always be inversely as the density; the increase of volume, therefore, which results from this transition is as 7 to 3.

It is generally supposed that all substances possess electricity in a certain state as to intensity and quantity, and have a certain ratio to the fluid in surrounding bodies, when in a non-electric state. When any substance receives an additional quantity, or has a part of its natural electricity abstracted, it is said to be electrified; in the former case positively, in the latter negatively.

From this opinion, it would necessarily follow, that when the zinc, passing from the metallic state to that of an oxide, obtains a volume represented by 7 instead of 3, the quantity of its electric fluid, in order retain the non-electric state, or in other words that of equilibrium, ought to be as 7, but it remains only as 3. The zinc therefore having a less amount of electricity in proportion to its bulk must be negatively electrified.

On the transition of the oxygen from the state in which it exists in water to that in the oxide of zinc. This effect is accompanied by an increase of density; the density of water being 1 and that of the oxide of zinc about 3. The spaces occupied by the oxygen before and after the transition are, as I have found by experiment, in inerse ratios, being in one instance 1 : 3 and in the other 3 : 1.

What influence then has this diminution of bulk on the electrical condition of the oxygen? The ratio which the oxygen when it forms water in combination with hydrogen, bears to surrounding bodies, as respects the quantity of the electric fluid necessary to obtain equilibrium, cannot be maintained when it combines with the zinc, and forms an oxide, the quantity being then as 3 : 1. The oxygen under such circumstances must be positively electrified, having the fluid in excess.

Hence then it will appear that the effect on the oxygen is the reverse of that on the zinc; the former being in the positive, and the latter in a negative state when the oxide of zinc is forming.

On the change effected by the union of the acid in solution with the oxide of zinc. As far as our information extends respecting the action of acids upon the oxides of metals, there can be no doubt that the compound thus formed has a greater density than the sum of the densities of the two constituents—

the acid and the oxide. When the union of the oxide of zinc with the acid of the solution takes place, there will be an increase of density and consequently a diminution of volume. The resulting compound containing an excess of fluid above equilibrium will be positively electrified.

On the electrical state of the metallic zinc in relation to the oxide of zinc formed from the same plate. The oxide of zinc immediately on its formation must contain, owing to the expantion of the metal itself, a less quantity of electricity than it should do in relation to surrounding bodies. It must therefore be in a negatively electrified state. The metallic zinc, remaining after the formation of the oxide, must be in a positive state, owing to what in the ordinary electrical language is called induction.

On the electrical state of the copper in relation to the zinc. The metallic zinc being positively electrified to the oxide of zinc, in must be in a negative state to all surrounding bodies, and among others to the copper with which it is in contact agreeably to the principles of induction.

On the transition of the hydrogen from the condition in which it exists in water to its uncombined gaseous state.[*] The hydrogen in the state of gas occupies a space which may be represented as 12,000; and in the state of water as 1. Hence it must follow that the hydrogen gas must contain less fluid than surrounding bodies, and is therefore electrified negatively.

It now becomes a question how the hydrogen can be brought to that positive state in which we know it to exist; as it is always attracted to the negative pole of the battery. It may be said that it is owing to the hydrogen being under the inductive power of the negative copper: but we have no evident reason why the copper should exercise that inductive power upon the hydrogen and not upon the other components of the arrangement. Induction may help us to a statement of the fact but gives us no assistance in an attempt to discover the cause.

From what has been already stated, it will be evident that metallic zinc, in its transition into the state of an oxide, undergoes expansion; that when oxygen is separated from its combination with hydrogen forming water and combines with the zinc, contraction will be the result; and the same result is produced upon the zinc and copper as well as when the oxide and acid are resolved into a saline compound. That there is some connexion between these changes and the electrical properties of the battery is undeniable; but I am not aware that any attempt has ever been made to trace out the connexion. It is true that an explanation is generally given by the assistance of the doctrine of induction, which supposes an electrified body to induce an opposite state upon the substances brought near it. Thus the poles of the battery may be considered as in opposite states, and all the electric actions in the solution as the result of their inductive influence. But it may be doubted whether the frequent use which is now made of the principles of induction may not retard the progress of electrical science. The word *induction* may be of use as connecting many isolated facts, but it gives us no insight into the cause of the phenomena attributed to the proximity of bodies to electrified substances. The frequent use of the word is therefore mischievous, as it prevents enquiry and serves only to hide our ignorance.

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[*] One cubic foot of water weighs 1000 ounces, which multiplied by 438 yields the number of grains 438,000. One cubic foot of hydrogen gas weights about 38 grains. 438,000 divided by 38 yields 11,526, which is the ratio of the density of water to hydrogen gas. This therefore is the amount of the expansion which hydrogen undergoes during its transition from the state of water to that of gas.

II. EXPLANATION OF THESE CHANGES ON THE THEORY OF VIBRATION.

1. In order to render this explanation more intelligible, we must suppose every body to be capable of undergoing vibration, consisting of a contracting, and an expanding stage; being positive and imparting the fluid during the former; and negative and receiving it during the latter.

As soon as the acid acts upon the zinc, the oxide formed will have its tendency to undergo the expanding stage increased, as already shown; thereby becoming highly negative. The fluid will be powerfully abstracted from the remainder of the metal zinc, which has therefore its tendency to undergo the contracting stage increased. While these two stages in each exist, a current as at *a*, fig. 68, Plate IX, will pass between them. When this current ceases, the opposite, the expanding stage commences in the zinc, it becomes negative, and absorbs the fluid from the copper which in turn becomes positive, thus current *b* is generated. When this ceases, the opposite, the expanding stage commences, it becomes negative, absorbing the fluid from the solution S which thereby becomes positive, generating the current *c*.

The general effect of this vibration will be, that a current will exist through the arrangement, from the zinc, through the solution, through the copper to the zinc again, whereby a complete circuit will be established.

It has been already mentioned, that the hydrogen at the moment of its transition from the state of water to that of gas, undergoing an expansion equal to 12,000, must be highly negative; which appears contrary to fact, as it is highly positive being attracted by the negative copper. The theory of vibration enables us to meet this difficulty. This gas being at the moment of its formation the most elastic body present, will absorb the fluid from the others, more particularly the solution, and will also, owing to its great elasticity, more readily subsequently undergo compression, become positive and give off its fluid to the negative copper.

If the above theory of vibration be true, it follows, that as the consumption of the fluid by the oxidizement of the zinc is not met by an equivalent production of the fluid within the battery, the action of the battery must depend upon its power of absorbing the fluid from surrounding bodies, rather than imparting it to them. This is highly probable from a most important experiment of Dr. Faraday, related in section 728 of his Experimental Researches. "If the acid be strong then a remarkable disappearance of oxygen took place; thus one made by mixing two measures of strong oil of vitriol with one of water, gave 42 volumes of hydrogen, but only 12 of oxygen." Now the inference which Dr. Faraday draws from this experiment that the duetoxide of hydrogen, or oxywater of Thenard, is formed, is highly probable. It may be proper to state that this compound consists of 1 hydrogen and 16 oxygen; water consists of the same, but the proportions different, 1 hydrogen and 8 oxygen. The former containing double the quantity of oxygen that the latter does. When water is decomposed, it yields 2 volumes of hydrogen and 1 of oxygen. In the above experiment 42 volumes of hydrogen are produced, but only 12 of oxygen instead of 21. What has become of the 9 volumes of oxygen? The only probable inference is, that it exists in combination with the water of the solution as the deutoxide. This compound can exist *only* at low temperatures, for as the temperature rises, oxygen is given off, and water alone remains. The presumption therefore is, that this compound which can exist at low temperatures *only*, is formed by the action of the battery, which is dependant upon its power of absorbing the fluid from surrounding bodies, rather than of imparting it to them. That this view of this action is the true one, the magnet furnishes further evidence. The lower the temperature, the stronger the power of the magnet, as Dr. Faraday has proved by an experiment related in the article Magnetism, section 51, published by the Society for the Diffusion of Useful Knowledge; and accordingly we find, that the wire of the battery, as when forming the common helix, converts a plain piece of steel as a common needle into a magnet; not by imparting the fluid, but by absorbing it. These two facts, these two analogous instances, appear to place the foregoing statement upon an incontrovertible basis, viz., that the battery forms oxywater, and also the magnet, by its power of absorption of the fluid.

I am aware that an objection has been made to the theory of vibration, because it cannot be proved by *experiment* to exist, and there is certainly not any experiment sufficiently delicate to determine its existence; but the great velocity with which these vibrations occur, preclude the possibility of direct observation; as thousands or even millions of them may occur within a second of time. The velocity of electricity has been estimated differently by different philosophers; but the determinations of Professor Wheatstone appear most worthy of dependance; viz. that the velocity of electricity through a copper wire exceeds that of light through planetary space.

It may now be stated that if the theory of vibration is true, it will follow, that as the positions and times of the two stages do not correspond, the *fluid* given out, and absorbed, during the contracting and expanding stages respectively, *will not be equal.*

Now, this, as respects sound, which is almost universally acknowledged to be dependant upon the vibration of the matter through which it passes, is known to be the fact. La Place, while investigating mathematically, the phenomena of sound, found that his *results* did *not correspond* with the *facts.* In Mrs. Somerville's Work, page 155, are these observations. "In dry air at the freezing temperature, sound travels at the rate of 1089 feet in a second, and at 62° of Fahrenheit its speed is 1123 feet in the same time. It was found, however, that the velocity of sound, determined by observation, exceeded what it ought to have been theoretically by 173 feet or about one sixth of the whole amount. La Place suggested that this discrepancy might arise from the increased elasticity of the air, in consequence of a development of latent heat, during the undulations of sound; and the result of calculation fully confirmed the accuracy of his views."

Were the name of La Place only known to us from the fact of his having made this observation, it would deserve immortality; for it explains to us, that the heat given out during the compression of the air, is not absorbed during the rarefaction necessarily accompanying it: for it is to be recollected, that the volume of the air remains not permanently contracted or compressed, but the same after the sound has passed through it, as before.

We are thus informed, not in the imperfect dialect of man, but in the universal language of nature, that this heat generated during the transmission of sound is a test of the presence of vibration.

Fig. 69, Plate IX. A B, an arrow denoting the direction of the force generating the vibration. C, the positions of the contracting stages of vibration of the matter through which the force acts. E, the positions of the expanding stage of vibration. Each vibration supposed to be constituted of the two stages, as from *a* to *b*. The arrows denote the fluid flowing in or out of the respective positions where the stages occur. The numbers denote the periods of time when the separate stages are supposed to occur.

From the diagram, it is evident, that as the positions and times of the stages differ, they cannot interfere: thus for instance, the contracting stage at 3 occurs too late to interfere with the expanding stage at 2, and too early to interfere with that at 4, because this last stage is not as yet supposed to be in existence. To this inevitable conclusion we are driven; that the expanding, cannot interfere with the contracting stages, because time and place do not permit it. That the expanding stage will require the fluid is undoubtedly true; but it is also as true, that the same fluid which is given out during the contracting stage is not all absorbed during the expanding stage. Hence the latent heat observed by La Place.

But here an objection may be started; that although the above observations may be justified from that of La Place, as respects sound, still we are not justified in extending the same by analogy, to the explanation of the phenomena of electricity. I believe we are fully justified. Sound, electricity, magnetism, light, and heat, are each and all in connexion with a force or motion through matter, and consequently also with its vibration. We have therefore yet to learn the existence of electrical phenomena independent of the presence of matter.

Heat therefore becomes a test of the vibration in matter. Thus the heat generated by a voltaic battery, denotes the vibration taking place in it; in the same manner as the heat observed by La Place was a test of the vibration accompanying the motion through air connected with sound; being that given out during the compression, but not absorbed during the rarefaction of the air, otherwise that heat could not have been observed.

From the foregoing investigation, two inferences are to be drawn, whose mutual influence upon the action of the voltaic battery is most important, and in the present imperfect state of electrical knowledge cannot be kept too constantly in view.

1*st. Inference.* That by the vibration existing in the battery, fluid is disengaged, whereby it manifests heating power in its action upon bodies.

2*d. Inference.* That the battery, owing to the consumption of the fluid in it, by the oxidizement of the zinc, absorbs the fluid from bodies exposed to its action.

These two inferences lead to an inconsistent result. The first inference implies that the battery imparts fluid to bodies exposed to its influence. The second, that it receives the fluid from them.

We accordingly find that batteries relatively to these two inferences, and their respective tendencies, may very conveniently be divided into two classes. One, including those whose action upon bodies is mainly dependant upon the heat they communicate to them. The other, those whose action is mainly electric and chemical. The first class includes *quantity;* the second *intensity* batteries.

As a *quantity* battery, Hare's calorimotor stands first. In it the influence of vibration predominates. It heats and deflagrates the metals and oxidizes them. It possesses no electric, no chemical action, giving no shocks, producing no decomposition. All this occurs, because, owing to the intensity of its vibration, its power of imparting the fluid to surrounding bodies overpowers that of abstracting it from them.

In an *intensity* battery, the power to absorb the fluid from bodies exceeds that of imparting it to them. It produces electric and chemical action. It gives shocks, it decomposes bodies, while its influence dependant upon vibration, is much inferior to that of a quantity battery.

We now see the reason why a small battery should produce such extraordinary effects upon electro-magnets, which are scarcely exceeded by the action of a larger battery. It is because the interference arising from the heat disengaged during the vibration is proportionably less in a small battery than in a large one; the electric action is therefore proportionably greater.

We now see the reason why the sustaining battery is so well adapted for electro-magnetic and decomposing purposes. It is because so little interference arises from the fluid given off during vibration, by which its electric and decomposing action might be impeded.

We now see the reason why batteries composed of metals and water alone, should, (as when employed by Mr. Crosse), produce crystals, thereby rivalling nature herself. It is because in such batteries the interference by the action of heat liberated by vibration is so slight, the electric action goes on undisturbed. No fact is scarcely better known, than that the abstraction of heat favours crystallization.

We also see the reason why the action of batteries made with sulphuric or nitric acids, must of necessity be short-lived. It is because by the intensity of their vibration and the accompanying production of heat, they counteract themselves.

III. THE DISTINCTION BETWEEN THE TERMS, QUANTITY AND INTENSITY, DETERMINED BY THE THEORY OF VIBRATION.

When the fluid given off, during the contracting stage is not counteracted, by being taken up, during the expanding stage, we get those effects which we term those of quantity: when it is counteracted, we get those of intensity.

Batteries of single pairs of plates are most proper for quantity. Those with numbers or series of pairs, for intensity. The cause of this will appear.

The calorimotor, the best sample of a quantity battery, consists of two plates, copper and zinc rolled up together, and acid. Now if we consider each separate plate, to be undergoing vibration, contracting on one side or end, and expanding on the opposite, it must be very evident that very little interference owing to the distance, can occur between them, and that this interference will be less as the size of the plate is greater. But in a battery composed of a series, interference will occur between the different stages, and to a greater extent and in a greater ratio, as the number of series is greater. This subject may be illustrated by a diagram.

Fig. 70, Plate IX. Q, intended to represent distance, as that of the surface of a plate belonging to a quantity battery. I, a series of plates, belonging to an intensity battery. C, positions undergoing the contracting stages of vibration. E, positions undergoing the expanding stages of vibration. Z and N, the positive and negative ends of the plates.

The arrows show the direction of the current supposed to pass along the plate or series. The numbers express the time when the vibration in each plate is supposed to occur.

Thus the fluid given out at C′, cannot be much affected by any action occurring at E′, owing to the distance; and any body near C′ may receive some of that fluid which C′ has in excess without any reference to the deficiency at E′. But in the series it is different. If, for instance, the fluid be given out at C. 5, although as in the former instance, it may occur without much reference to the expanding stage at E. 5, still it is more likely to pass to E. 6, where the fluid is deficient than to any body near it, as it would in the former instance. This teaches us why, from the action of a quantity battery, heat ought to result; and why it is less likely in an intensity battery. To this conclusion we come, that in a single pair, heating effects are in a greater ratio to the electrical and chemical; and in a series, in a less ratio; owing to the power of a single pair to impart fluid to bodies, agreeably to the first inference we draw; and that of a series to abstract it from them, agreeably to the second inference.

Much confusion has arisen from inattention to this twofold action of the voltaic battery, and much surprise has been manifested that quantity batteries should show such enormous powers when acting upon good conductors, and at the same time, such weak electrical powers; when it was supposed that the only difference between a quantity and an intensity battery was, that in the former instance the electric fluid was diffused over a larger surface, and in the latter more concentrated.

This we now see is not the fact. The phenomena of the battery when imparting the fluid to bodies, are analogous if not identical with those of heat. The term quantity as applied to the electrical action of the battery is altogether improper, for instead of expressing a similar force differing in degree merely, it is an antagonist force, exactly in the same manner as we are compelled to consider the force of heat, antagonist to that of crystallization. Hence has arisen the uncertainty and confusion by which the progress of electrical science has been retarded. Hence the opposing theories, which by their conflict have shown that neither were right, because both had equally confounded antagonist, with forces differing merely in degree. The history of electrical controversy is almost entirely constituted of the details of this lamentable mistake.

The following observations in Donovan's Galvanism, page 251, are highly instructive. In objecting to the hypothesis of Volta the following experiment is related. "With a battery of forty pairs of plates each 18 inches square, the effect on the gold leaf electrometer was barely sensible, yet two pairs of plates will ignite and fuse some inches of platina wire." It is afterwards observed "but if the wire be fused by the intensity of the electricity, or what is the same by the condensation of a large quantity upon a small surface, how are we to account for the non-effect on the electrometer, for the leaves separate with the feeblest intensities?"

The above is a sample of the evils arising from the confounding the heating with the electrical power of the battery. We now know the reason why two pairs of plates should ignite some inches of platina wire, while forty plates should barely influence the electrometer. It is because these two actions depend upon antagonist forces.

IV. VARIOUS OBJECTIONS MADE TO THE FOREGOING THEORY.

OBJECTION 1. *Want of experiments to show the changes of form in the battery referred to, to take place.* It would be difficult to show the changes as they are assumed to occur in the battery, but we have no reason to suppose that the solution of zinc formed by the action of the battery differs from the same product formed in any other way, as by forming the oxide from the metal zinc and dissolving this oxide in the acid employed: the result being the same in either case. That the metal zinc undergoes expansion in forming the oxide, can be very readily shown by taking the specific gravities of the metal and oxide, and by adding a given weight of each to water in a common phial and observing the difference of elevation by a graduated scale.

OBJECTION 2. *Chemical action not the cause of all electrical action.* Chemical action appears to be the cause of the electric action of the battery, for we find, that if the former be diminished, the latter is reduced. But we are not to suppose that chemical action is the sole cause of electric effect. The present state of our electrical knowledge is far from justifying us in drawing such an inference. The cause of electric action may be more deeply seated, as it appears to depend upon the vibration of matter, which by disturbing the equilibrium of the quantity of fluid, produces electric effect. If, therefore, we are to found electric action upon the basis of chemical action, we must suppose that the arrangement of the atoms of matter during its vibration undergoes a chemical change. This probably may turn out to be the truth, but with our present knowledge is incapable of demonstration.

OBJECTION 3. *That electric phenomena may be explained by the examination of the properties of matter, independently of any fluid.* Although it might be possible to explain the phenomena of the battery, agreeably to the commonly received laws of matter, yet it appears much more consistent with the fertility and variety which nature employs in her operations, that such a species of matter called an electric fluid should exist. The balance of probability seems strongly in its favour. If we suppose a matter whose parts are so intensely small, that the attractive force between themselves and surrounding matter exceeds that within themselves, we appear to possess all that we require to explain the phenomena dependant upon an electric fluid. Such matter must be highly elastic, highly diffusive, strongly disposed to pass in the direction of least resistance.

OBJECTION 4. *That if two plates of zinc, one simple, and the other amalgamated, be placed in the same acid solution, and connected with the galvanometer, the current generated would be in the direction different from what it ought according to the preceding views.* In the foregoing paper I have confined my observations to the voltaic battery as commonly arranged. In this experiment, I see nothing subversive of the theory of vibration, but I think confirmatory of it, if the thermo-electric condition of the arrangement be examined. We will shortly reconsider this objection.

No theory hitherto advanced has explained the many anomalies attending the action of the voltaic battery upon the wire and needle when forming the galvanometer. Thus the deflections of the needle during the same experiment frequently occur in opposite directions. The theory of vibration enables us to meet these anomalies, and to show that they constitute its necessary consequence.

In Mr. Sturgeon's Experimental Researches are many of these anomalies. Thus the deflections of the needle when the acid employed is weak is frequently the reverse of that occurring when strong. The experiments in paragraphs 11 and 12 strongly illustrate this. A small wooden box B, fig. 17, Plate IX, divided in the middle by a diaphragm *d*, thus forming two chambers; in the one A is acid; in the other W is water. In each is immersed a plate of copper *a* and *b*, each connected by the wires *w w*, with the galvanometer *g*, of which *n* is the needle. In experiment 11, 40 or 50 parts of water to one of nitrous acid are employed; in experiment 12, equal parts. In experiment 11, the deflection of the needle denotes that the current *o o*, passes from the plate *b*, in the chamber W, to plate *a*, in the chamber A. In experiment 12, the deflection denotes that the current *v v* is passing in the opposite direction. Mr. Sturgeon subsequently observes that when strong acid is employed, the deflection at the commencement is the same as with weak acid, but after some time undergoes this vicissitude.*

The theory of vibration teaches us the cause of this difference in the direction of the two currents, and the consequent difference in the deflection of the needle. If, as in experiment 11, the acid employed be weak, the oxidizement of the plate

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* Sturgeon's Experimental Researches, p. 22, 23.

a, will proceed slowly, the resulting vibration will be weak, the quantity of fluid liberated will be small, and will consequently offer little interruption to the current *o o*, generated by the above oxidizement. But when on the contrary, as in experiment 12, the acid in A is strong the resulting vibration will be strong, the quantity of fluid liberated will be large, the current *o o*, will be overpowered, and the current *v v*, flowing in the opposite direction, will be established. We also see, why, as Mr. Sturgeon has observed, *time* is essential even with the strong acid for the development of the contrary current *v v*. It is because the latter is the creature or effect of the former current.

If we substitute two plates of zinc, one plain *a*, and the other amalgamated *b*, for those of copper, and remove the diaphragm, employing the acid solution alone, the deflection of the needle is such as denotes the existence of the current *v v*, not *o o*, although the negative plate *a* becomes oxidized, the positive plate *b* being scarcely acted upon. This is in direct opposition to the chemical theory of electricity. This experiment appears to be in accordance with the theory of vibration, and its generated current, *v v*. The amalgam of zinc does not, as might at first be supposed, occupy the place of the copper, the negative metal in the standard battery; for we are informed by Jacobi (Annals of Electricity, pages 429 and 431) that the amalgams are positive to their constituent metals. According to this view, the current ought to pass in the direction *v v*, as it is found to do in this experiment.

Objection 5. *That when water instead of acid was employed in the calorimotor, by its action, crystals had been produced by Mr. Crosse.* In this experiment owing to the weak vibration, the heat disengaged will be so small as scarcely to interfere with the action of the battery abstracting the fluid from bodies, owing to the oxidizement of the zinc. Hence the crystals.

Note.—Since the commencement of the foregoing paper, I have met with some views of Mr. Karsten, of Berlin, of the effects of electricity by contact, which are highly in accordance with those advanced. That metals and probably all solid bodies become positively electrified when immersed in fluids. That a solid partially immersed in a fluid, acquires electric polarity; the part not immersed being negative, and the other positive. That solid bodies differ greatly in their electro-motive power in regard to the same fluid; and this difference is the true cause of the electric, chemical, and magnetic action in the galvanic circuit.

LV. *Specification of a Patent for the application of Electro-Magnetism to the propelling of Machinery; granted to* THOMAS DAVENPORT, *of Brandon, Rutland County, Vermont, February,* 1837.*

Be it known, that I, Thomas Davenport, of the Town of Brandon, in the County of Rutland, and State of Vermont, have made a discovery, being an application of magnetism, and electro-magnetism, for propelling machinery, which is described as follows, reference being had to the drawings of the same, making part of this specification.

The machine for applying the power of magnetism, and electro-magnetism, is described as follows; the frame A, fig. 72 Plate IX, may be made of a circular, or any other figure divided into two, or more platforms, B and C, upon which the apparatus rests; and of a size and strength adapted for the purpose intended. The galvanic battery, D, is constructed by placing plates of copper and zinc, alternately, of any figure, in a vessel of diluted acid: there are two conductors, H and I, one from the copper and one from the zinc, in the vessel D, leading to, and in contact with copper plates, K and L, placed upon the lower platform. These plates, or conductors, are made in the form of a segment of a circle, corresponding in number with the artificial magnets hereinafter described; they are placed around the shaft, detached from one another and from the shaft, having a conductor, leading from the copper plate of the battery, to one of said plates on the lower platform, and another conductor leading from the zinc plate of the battery, to the next plate on the said lower platform, and so on alternately (if there be more than two plates on said lower platform) around the circle.

The galvanic magnets, M, N, O, P, are constructed of arms, or pieces, of soft iron in the shape of a straight bar, horse shoe, or any other figure, wound with copper wire *q*, first insulated with silk between the coils: these arms project on lines from the centre of a vertical shaft R, turning on a pivot, or point, in the lower platform; said copper wires *q*, *q*, extending from the arms parallel, or nearly so, with the shaft down to the copper plates, K and L, and in contact with them.

The galvanic magnets, are fixed on a horizontal wheel of wood V, attached to the shaft.

The artificial Magnets S, T, are made of steel, and in the usual manner. They may be of any number, and degree of

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* From the Journal of the Franklin Institute.

strength, and fixed on the upper platform, being segments of nearly the same circle as this platform; or if galvanic magnets are used, (which may be done,) they may be made in the form of a crescent, or horse shoe, with their poles pointing to the shaft.

Having arranged these artificial magnets, on the top of the upper circular platform, there will be a corresponding number of magnetic poles—the north marked 5, and the south pole 6. Now we will suppose the machine to be in a quiescent state; the galvanic magnet, No. 1, being opposite the south pole of the artificial magnets, the galvanic magnet, No. 3, will, of course, be opposite the south pole, No. 6, and the galvanic magnets, No. 2 and 4, will be opposite each other, between the poles just mentioned.

There being a corresponding number of copper plates, or conductors, placed below the artificial magnets around the shaft, but detached from it, as well as from each other, with wires leading from the galvanic magnets to these plates, and in contact with them, as before described, these wires will stand in the same position, in relation to the copper plates, that the galvanic magnets stand to the artificial magnets, but in contact with the plates.

Now in order to put the machine in motion, the galvanic magnet, No. 2, being charged by the galvanic current passing from the copper plate of the battery, along the conductors and wires, becomes a north pole, whilst, at the same time, the magnet, No. 4, is charged by the galvanic current passing from the zinc plate of the battery, and becomes a south pole; of course the south pole of the artificial magnet, No. 6, will attract the north pole of the galvanic magnet, No. 2, and will move it a quarter of a circle; the south pole of the galvanic magnet, No. 4, being at the same time attracted by the north pole, No. 5, causes the same magnet, No. 4, also to perform a quarter of a circle: the momentum of the galvanic arms will carry them past the centre of the poles, No. 5 and 6, at which time the several wires from the galvanic magnets, will have changed their positions in relation to the copper plates or conductors:—For instance, the north pole, No. 2, having now become a south pole, by reason of its wires being brought in contact with the conductors of the zinc plate, and No. 4 having, in like manner, become a north pole, its wire having changed its position from the zinc plate to the copper plate, the poles of the galvanic magnets are, of course, now repelled by the poles that before attracted them; and in this manner the operation is continued, producing a rotary motion in the shaft, which motion is conveyed to machinery, for the purpose of propelling the same.

The discovery here claimed, and desired to be secured by Letters Patent, consists in applying magnetic and electro-magnetic power, as a moving principle for machinery, in the manner above described, or in any other substantially the same in principle.

THOMAS DAVENPORT.

Remarks by the Editor of the Journal of the Franklin Institute.

The subject of the foregoing specification is one of great interest, and it has arrested a corresponding portion of public attention; we are likely soon, therefore, to have the question solved, whether this new power can be advantageously applied to the propelling of machinery as a subititute for the steam engine. Most of our readers, it is presumed, have seen Professor Silliman's notice of Mr Davenport's machine, published in the Journal of Science, in April last, which contains much information respecting the attempts which had been made for the producing of motion by electro-magnetic apparatus. Since that period, advice has been received from Europe, showing that experiments upon this subject are in progress under the direction of some of the most distinguished philosophers in various portions of that quarter of the globe.

We do not know by whom, or at what date, the first successful experiment of producing a direct rotary motion, by the electro-magnetic apparatus, upon a principle analogous to that upon which Mr. Davenport has proceeded, was performed. As early, however, as June, 1833, an article appeared in the London Mechanics' Magazine, proposing such an apparatus, and giving a figure of one which it was supposed, would answer the purpose; a supposition which was, undoubtedly, well founded. Not long after this, Mr. Saxton, we believe, produced a rotative machine by electro-magnetism, but we are not informed respecting its particular arrangements.

The history of the production of the machine patented by Mr. Davenport, is the history of the successful efforts of an individual who, to an indomitable perseverance, must have superadded extraordinary natural abilities. His business is that of a blacksmith, and his advantages in point of education were not greater than usually falls to the lot of persons in country places, engaged in such pursuits. Accident brought to his notice one of Professor Henry's electro-magnets, which he eagerly purchased, under a conviction that he could render it available as a motive power: this was in the year 1833, and in July, 1834, he had so far succeeded as to produce a rotative machine; and this he effected in a country village, unaided by scientific knowledge, by books, or by the encouragement of men of superior attainments, or with kindred spirits. Whatever may be the final result of his labours, his merits are of a high order, and he has proved himself well worthy of the most splendid success. Should his machine finally accomplish that which he and many of his friends anticipate, its value will be incalculable, for although he may have been superseded in Europe, his claim as inventor will undoubtedly prove valid in his own country, and ambition need not carry him beyond it. We have twice seen his machine in operation, formerly in New York, and recently in Washington, where it was exhibited to the President, and the Heads of Departments. So far as the evidence of a model is to be taken, its performance is quite satisfactory; and Mr. Davenport is now occupied in constructing one which is intended to drive a Napier Press, requiring a two-horse power. This, should it succeed, will be a fair test of its value, and we confess that, although our expectations do not generally partake of the sanguine in such matters, not only our hopes, but we may say our confidence, has increased as we have become acquainted with the progress of the experiments which are being carried on.

We are well aware, that should it be eventually proved, that an available power may be obtained, which may be substituted for that of steam, its adoption would depend entirely upon its economy, with respect to which we cannot have satisfactory data until a machine of several horses power shall have been produced; the probability, however, is, that the cost of operating the electro-magnetic apparatus, will be much below that of the steam engine.*

LVI. *Extraordinary case of electrical excitement, with preliminary remarks by the Editor.*†

The facts stated below, by my request, were kindly communicated for this Journal by Dr. Willard Hosford, a respectable physician of Oxford, New Hampshire, the place where the occurrence happened. Being in that place in September, and finding the belief in the facts to be universal, particularly on the part of persons of judgment and science, (as at the neighboring University, Dartmouth, at Hanover, eighteen miles south,) I became desirous of preserving a record of them.

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* A general ontline of the history of Electro-magnetic Engines will be given in the next number of the "Annals."

† From Silliman's American Philosophical Journal.

Dr. Hosford remarks in the letter accompanying his communication, that abundant evidence from the most intelligent persons is at hand for the support of every point in the case. He observes, also, that the appearance of the aurora during which the electrical excitement of the lady took place, "was precisely the same as that described by some gentlemen at New Haven."

Speaking of it Dr. Hosford adds, that "the heavens were lighted with a crimson aurora of such uncommon splendor, as to excite no ordinary emotions in every observer, and we had, he observes, in addition, an electrical exhibition much less dazzling, but more singular and to the parties concerned more interesting."

A lady of great respectability, during the evening of the 25th of January, 1837, the time when the aurora occurred, became suddenly and unconsciously charged with electricity, and she gave the first exhibition of this power in passing her hand over the face of her brother, when, to the astonishment of both, vivid electrical sparks passed to it from the end of each finger.

The fact was immediately mentioned, but the company were so sceptical that each in succession required for conviction, both to see and feel the spark. On entering the room soon afterward, the combined testimony of the company was insufficient to convince me of the fact until a spark, three fourths of an inch long, passed from the lady's knuckle to my nose causing an involuntary recoil. This power continued with augmented force from the 25th of January to the last of February, when it began to decline, and became extinct by the middle of May.

The quantity of electricity manifested during some days was much more than on others, and different hours were often marked by a like variableness; but it is believed, that under favorable circumstances, from the 25th of January to the 1st of the following April, there was no time when the lady was incapable of yielding electrical sparks.

The most prominent circumstances which appeared to add to her electrical power, were an atmosphere of about 80° Fah., moderate exercise, tranquillity of mind, and social enjoyment; these, severally or combined, added to her productive power, while the reverse diminished it precisely in the same ratio. Of these, a high temperature evidently had the greatest effect, while the excitement diminished as the mercury sunk, and disappeared before it reached zero. The lady thinks fear alone would produce the same effect by its check on the vital action.

We had no evidence that the barometrical condition of the atmosphere exerted any influence, and the result was precisely the same whether it were humid or arid.

It is not strange that the lady suffered a severe mental perturbation from the visitation of a power so unexpected and undesired, in addition to the vexation arising from her involuntarily giving sparks to every conducting body, that came within the sphere of her electrical influence; for whatever of the iron stove or its appurtenances, or the metallic utensils of her work box, such as needles, scissors, knife, pencil, &c. &c., she had occasion to lay her hands upon, first received a spark, producing a consequent twinge at the point of contact.

The imperfection of her insulator is to be regretted, as it was only the common Turkey carpet of her parlor, and it could sustain an electrical intensity only equal to giving sparks one and a half inch long; these were, however, amply sufficient to satisfy the the most sceptical observer, of the existence in or about her system, of an active power that furnished an uninterrupted flow of the electrical fluid, of the amount of which, perhaps the reader may obtain a very definite idea by reflecting upon the following experiments. When her finger was brought within one sixteenth of an inch of a metallic body, a spark that was heard, seen, and felt, passed every second. When she was seated with her feet on the stove-hearth (of iron) engaged with her books, with no motion but that of breathing and the turning of leaves, then three or more sparks per minute would pass to the stove, notwithstanding the insulation of her shoes and silk hosiery. Indeed, her easy chair was no protection from these inconveniences, for this subtle agent would often find its way through the stuffing and covering of its arms to its steel frame work. In a few moments she could charge other persons insulated like herself, thus enabling the first individual to pass it on to a second, and the second to a third.

When most favorably circumstanced, four sparks per minute, of one inch and a half, would pass from the end of her finger to a brass ball on the stove; these were quite brilliant, distinctly seen and heard in any part of a large room, and sharply felt when they passed to another person. In order further to test the strength of this measure, it was passed to the balls by four persons forming a line; this, however, evidently diminished its intensity, yet the spark was bright.*

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* It is greatly to be regretted that the spark had not been received into a Leyden bottle until it would accumulate no longer, and then transferred to a line of persons to receive the shock.—Ed.

The foregoing experiments, and others of a similar kind, were indefinitely repeated, we safely say hundreds of times, and to those who witnessed the exhibitions they were perfectly satisfactory, as much so as if they had been produced by an electrical machine and the electricity accumulated in a battery.

The lady had no internal evidence of this faculty, a faculty sui generis; it was manifest to her only in the phenomena of its leaving her by sparks, and its dissipation was imperceptible, while walking in her room or seated in a common chair, even after the intensity had previously arrived at the point, of affording one and a half inch sparks.

Neither the lady's hair or silk, so far as was noticed, was ever in a state of divergence; but without doubt this was owing to her dress being thick and heavy, and to her hair having been laid smooth at her toilet and firmly fixed before she appeared upon her insulator.

As this case advanced, and supposing the electricity to have resulted from the friction of her silk, I directed (after a few days) an entire change of my patient's apparel, believing that the substitution of one of cotton, flannel, &c., would relieve her from her electrical inconveniences,* and at the same time a sister, then staying with her, by my request, assumed her dress or a precisely similar one; but in both instances the experiment was an entire failure, for it neither abated the intensity of the electrical excitement in the former instance, or produced it in the latter.

My next conjecture was, that the electricity resulted from the friction of her flannels on the surface, but this suggestion was soon destroyed when at my next visit I found my patient, although in a free perspiration, still highly charged with the electrical excitement. And now if it is difficult to believe that this is a product of the animal system, it is hoped that the sceptics will tell us from whence it came.†

In addition to the ordinary appurtenances of a parlor, it may be proper to add, that the lady's apartment contained a beautiful cabinet of shells, minerals, and foreign curiosities.

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* This could hardly have been expected from non-conductors; we are informed that the lady was relieved of the electricity by a free communication with the earth by a good conductor, in the manner of a lightning rod, as by touching the stove and its connection with the earth through the medium of the chimney..—Ed.

† It appears to be Dr. Horsford's opinion, that the electricity was not caused by the aurora that was coincident with its first appearance, but that it was, in some way, an appendage of the animal system.—Ed.

This lady is the wife of a very respectable gentleman of this place; she is aged about thirty, of a delicate constitution, nervous temperament, sedentary habits, usually engaged with her books or needlework, and generally enjoying a fine flow of spirits.

She has, however, never been in sound health, but has seldom been confined to her bed by sickness even for a day.

During the past two years she has suffered several attacks of acute rheumatism, of only a few days' continuance, but during the autumn, and the part of winter preceding her electrical development, she suffered much from unseated neuralgia in the various parts of her system, and was particularly affected in the cutis vera, in isolated patches; the sensation produced being precisely like that caused by the application of water heated to the point a little short of producing vesication; in no instance, however, did it produce an apparent hyperæmia, but about the last of December a retrocession took place of this peculiar irritation, to the mucous membranes of the fauces, œsophagus, and stomach, there producing a very apparent hyperæmia, and attended, during the exacerbations, with burning sensations that were torturing indeed; and it was for the relief of these symptoms that medical means were used, but it was found no easy matter to overcome this train of morbid action.

It was nearly immaterial what medicines were used; no permanent relief was obtained, and no advantage resulted from the use of the alkalies, or their varied combinations. In a few instances, a dose of the acetate of morphine was given to secure a night's rest, but she seldom made use of an anodyne.

The effervescing soda draught being very acceptable was freely given—from which, in addition to a rigid system of dietetics, the influence of the opening spring, and the vis medicatrix naturæ, relief came of her electrical vexations, of most of her neuragia, and other corporeal infirmities, and to this time, a much better state of health has been enjoyed than for many years.

LVII. *Note on a kind of Acarus presented to the Academy, at the Session of the* 30*th of October, by* M. ROBERTON,* *to whom Mr. Crosse had communicated it; by M. Turpin.*†

EXTRACT.

The *acarus* of Mr. Crosse, a single specimen of which was preserved in spirits of wine contained in a small phial, offered for our examination the following characters.

Seen by the naked eye, when enclosed in the vessel, it appeared merely as a whitish speck, which from its specific gravity always occupied the bottom of the phial.

A small oval body, bristled with long and diverging hairs, is seen by means of a lens.

Taken out of the alcohol, dried as much as possible, and then placed between two plates of glass, in a fine bed of white varnish, so as to render all its parts more transparent, and consequently more easy to study, we submitted it to the action of the microscope, which magnified about 280 times.

By this mode of observation we perceived that the body was of an oval form, the belly slightly flattened, and the back very round, particularly towards the hind part of the body.

The skin of the back appeared chagrined, or as strewed with an infinite number of very small tubercles, of which a certain number, larger than the others, distributed here and there, serve as a base or bulb for long hairs or silks, which are placed in all directions, and the greater part of which are at least as long as the body of the animal.

All these hairs fixed and raised on the protuberant back of this kind of *acarus*, give it the appearance of a microscopic porcupine, to which the elongated snout also contributes.

We have not been able to discover, by transparency, any trace of stomach, ovary, or lateral pulmonary organs.

The anus is feebly distinguishable by a slight inclination situated in the direction of the median line, and the posterior part of the abdomen.

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* Immediately after the reading of this note, M. Roberton wrote to us that the *Acarus* presented by him to the Academy, had not come directly from Mr. Crosse to him, but by M. Buckland, to whom Mr. Crosse had given several specimens; which, as we might suppose, offered more females than males, following the natural custom of this kind, as also of many others; which accounts for the single individual presented to the Academy, being a female ready to lay the egg she contained.

† From the Comptes Rendus, &c., November 13, 1837. Translated by Mr. J. H. Lang.

But a large oval egg is very clearly seen, like these, often to the amount of two, three, or even four, which are seen in the tranparent body of the domestic female acari of cheese and meal,* and in those of the same sex, of the *acarus* of the human mange;† as is shown in the drawing we have the honour to lay before the Academy.

This egg, both ends of which are equal, and whose length is 1-7th of a millimetre, is so far remarkable, that it is found in the single individual sent by Mr. Crosse, as if chance had intended to furnish us with a material proof of the manner of reproduction, well known among the Acarians, of this *acarus* which is thought to be produced at pleasure, without a mother preceding it, and by the aid *only* of elementary molecules floating in the space.

From the front part of the body emanates an elongated snoutish head, very difficult to study in its composition; but in which we can clearly see an upper lip sloping to its extremity, under which is situated a tongue in the form of a stiletto; and beneath which again, but situated laterally, are perceived two large moveable jaws, pointed and slightly bent from without and within.‡ Further in, and directed in the same way, appear two palpes, shorter than the jaws, and almost hidden by them and the lip which protects them.

Having only a single individual at our disposal, it was impossible for us to prove the existence of an under lip, so large and apparent in the *acarus* of the human scab.

We have not been more fortunate with regard to the two small smooth eyes, situated on the neck of the species of this genus.

From a sort of elongated narrow breast bone proceed eight appendicular locomotive members, joined and directed, the four anterior ones before, and the four posterior ones behind. They are all composed of the same number of pieces; but one thing which may be remarked in many insects, *arachnides* and *crustaces*, the two pair of anterior paws are shorter, thicker, and more robust than the inferior ones, which are longer, and at the same time more slim. This difference, scarcely perceptible in the acari of cheese and meal, is very great in the *acarus* of the human scab, when, without an attentive examination, we should be almost tempted to see different organs in the two posterior pair of paws, which, in fact, are only rudimentary.

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* *Acarus siro.*—Fab.

† *Acarus scabici.*—Fab.

‡ It is probable these jaws terminate in a point.

Each of the eight limbs of the acarus sent by Mr. Crosse, was formed of seven joints, exclusive of the tarsus; a first, which is triangular and may be considered as the hip; a second and third, longer than the hip; a fourth, longer than the two preceding ones; a fifth, shorter than the fourth; a sixth and seventh, longer and thinner than the others, and the latter of which terminated by a small transparent tarsus, which appeared to us bi-lobed, and provided with a single claw bent underneath.

On the upper edge of the joints, with the exception of that forming the hip, were found one or two straight hairs.

The real length of the body and head was half a millimetre.

The arachnide of Mr. Crosse appears to constitute a new species of the *acarus* race. The species described and figured, to which it nearest approaches, are those of cheese and meal, and perhaps more particularly the *acarus dimidiatus* of Hermann.* It differs from the two first by the absence of the false corslet, by the two longest and most slender joints which precede the tarsus, the form of the body which is more oval, shorter, and more bent, and finally by the numerous and long hairs which cover the whole of the back. It is distinguished from the *acarus dimidiatus,* which has a spherical body, with an appearance of corslet more coloured than the rest of the abdomen, by the want of small short hairs which cover the surface of the eight appendicular limbs of the latter; but it approaches it by the numerous hairs which cover in rays the whole of the back.

Thus far we have strictly confined ourselves to natural history. We have observed, described, drawn, counted, and measured all the constituent parts of this little animal. We have, by this means, proved that the small phial presented to the Academy by M. Roberton, contained the animalcule or *acarus* spoken of; which also might be seen at a simple view as a whitish point.

We may now say something on the singular origin, and more on the singular fabrication of so complicated an animal, although microscopic, so exalted in the organic scale, and whose structure, as we have seen, is composed of 1st. a body; 2d. a head, formed of two lips, two jaws, two palpé, a sucker, a mouth, and two eyes; 3d. a stomach and an anus; 4th. two lateral pulmonary organs;† 5th. an ovary, containing eggs, in the female ones; 6th. eight appendicular limbs, each composed of eight joints, including the tarsus; 7th. a skin bristled with long and numerous hairs.

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* Hermann. *Mem. apt.* VI. 4.

† The contraction of the animal, for some time immersed in alcohol, prevented us from distinctly seeing the eyes and pulmonary organs.

As we see it, we could scarcely increase the complication of this animal, though in addition to what we have just said, there are distinct sexes; among which there is a coupling and fecundation necessary for the reproduction of individuals of the species, and consequently we must admit there are genital organs; and, finally, the females make and lay eggs, whence the young ones are hatched, which at first have only six claws, until, changing their skin, they show two more, which were developed by degrees under this cutaneous skin.

Before attempting to make animals as complicated as the *acarus*, let us only try to create or obtain globules of Protespheries, and filaments of Protonemes;* the two organic productions, which appear to us the most simple of the organic reign, which are the commencement of organization; and which mark the moment that matter globulizes and arranges itself, to serve the next instant for the formation or texture of different tissular masses of all other beings, whether vegetable or animal.

In these extremely simple globules and filaments, no interior granulation can be perceived, able to serve for their reproduction. Hence we may, perhaps, believe that these two sorts of beings, really elementary for those of a superior order, are organized productions, formed immediately from the matter. But who can assure us that these Protospheries and Protonemes do not contain reproductive globules, which escape the action even of our most powerful microscopes; or also, which will almost come to the same thing, who can say that these vegetables, so simple and at the same time so small, are not divided into particles immediately the life of association abandons them; so that each of the particles, animated with a new and independent life, becomes a sort of slip which reproduces the species. If these are only suppositions, they have at least the merit of being in perfect accordance with what everywhere takes place, besides these two single productions.

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* *Protospharia simplex.* Turp. *Protonema simplex.* Turp. *Dictionnaire des Sciences Naturelles.* Botanic Atlas, Vol. II. Plate III. At present we possess, in a living state, a considerable number of filamentous Protonemes, which vegetate confusedly with *Hamatococcus* and *Heterocarpello geminata.*

We shall do ourselves great pleasure in showing them under the microscope, to persons who may wish to see one of the two organized beings, the most simple of the organic reign, with regard to man, which is its most compound.

The Protonemes are complete beings in their species: they are not a thallus or stock preceding or preparing a terminal fructification, such as, for example, the byssus of the mushroom.

All our microscopic studies on organized beings, whether vegetable or animal, the smallest in their dimensions, as well as the simplest in their structure, have always shown us that their mode of reproduction was entirely submitted to the power of a similar mother which, *alone,* can, by placing her nutritive materials in space, conceal herself in a destined germ, by insulation, for the reproduction and maintenance of the species.

It is thus, that, in proportion as we further study organized beings comparatively, and as we approach the smallest by the aid of the microscope, we see these numerous presumed spontaneous generations successively disappear, sprung from phantoms which could not support the light of a true and constant observation.

From our own knowledge, acquired by a long series of labours, in organization and physiology, we should say that the means which Mr. Crosse has employed, even supposing them in this case indispensable to the appearance of the animal, have only been simple stimulants, which, like those which excite and favour the germination of a grain of wheat, have hastened the hatching of the eggs, *similar to those contained by the female individual sent by Mr. Crosse himself;* eggs which were lain or brought on the surface of the Vesuvian stones used in the experiment.

Ignorant of the works written by Mr. Crosse, on the artificial and voluntary production of his *acarus,* we do not know whether the animal comes from the experiment in its most complete state, or whether, as would be more in accordance with the law presiding over the development of all organized beings, it passes through all the phases of developments and metamorphoses we so well know among all species of *acarus.* If, in the experiment, it begins by being only a point, then a globule, then an egg, afterwards a young *acarus,* having as yet only six claws, and finally a perfect acarus with eight, male or female without eggs, or containing some like that in the figure which we have had the honour to show the Academy. But in this way of viewing the fabrication of Mr. Crosse's acarus, there would still remain a very great difficulty—that of knowing where and how these animals, naturally so voracious, would find the nourishment necessary for their development; for organized beings can only increase in size and weight, by taking the nutritive matter which they find about them, and assimilating it themselves, by a mysterious power which belongs to them.

Explanation of the Plate.

Plate X.—The animal seen under the microscope.

LVIII. *Facts and Observations for the purpose of illustrating a Theory, intended to connect the Operations of Nature, upon general principles.* By Paul Cooper, Esq.

46. In my last paper* I endeavoured to show that many of the phenomena of common electricity are connected with what I have there called the resistance to derangement; but the effects of this resistance are not confined to common electricity, the same principles having the most important influence, both in galvanic and electro-chemical action.

47. "It had long been suspected, rather than proved, that a feeble degree of electricity is evolved by the contact or collision of different metals: but this important fact was established in the clearest manner by Volta, about the year 1801. The apparatus he employed in his investigations on this subject, consisted of two discs, the one of zinc, the other of copper, rather more than two inches in diameter, ground perfectly plain, and having in their centres insulating handles perpendicular to their surface, by means of which the plates could be brought into contact, without being actually touched with the hand. With this precaution the discs were made to approach till they touched one another; they were then separated, by keeping them parallel as they were drawn back. The electricity they possessed after this separation was then examined by means of a condenser; and it was constantly found that the copper disc charged the condenser with negative, and the zinc disc with positive electricity. Thus it was established as a general fact, that these two metals, insulated, and in their natural state, are brought, by mutual contact, into opposite electrical states; the zinc acquiring positive electricity, and the copper becoming, in an equal degree, negative."†

48. This experiment, in which an electric current is produced by the contact of dissimilar metals, displays the foundation of Galvanism; as the contact of the glass cylinder

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* Annals, Vol. I. p. 444.

† Library of Useful Knowledge, Electricity, 203.

with the rubber of an electrical machine, bodies possessing different electrical forces, exhibits the foundation of electricity (15): the only difference in the two experiments is, that in the former, both the bodies are conductors, and in the latter, one is a conductor and the other a non-conductor of the fluid, the equilibrium of which is, in both cases, disturbed by the forces which are brought into action.

49. When a plate of zinc is brought into contact with a plate of copper, the former being positive to the latter, the atmospheres (10) of the atoms of the two plates, whatever may be their number, will assume the form represented by sections, perpendicular to the surface of the plates, in fig. 76, Plate XI.*

The light which forms the atmospheres of the atoms of the zinc plate flows towards the copper; because the atmosphere of the atom of zinc, C, meets a force of less resistance in the atmosphere of D, the atom of copper with which it is in contact, than the force by which it is propelled towards it by the atmosphere of B, with which it is also in contact; but the atoms, A, B, C, &c., of the zinc plate are so connected with each other, that no change can take place in the atmosphere of C without a corresponding change in the atmospheres of A and B; and the whole will simultaneously assume the form represented in the figure. It is necessary to observe that the light which forms these atmospheres, although it has the power to move from one side to the opposite side of its own atom, so as to render these sides positive and negative in comparison with each other, cannot quit the atom to which it belongs; except in a trifling degree, under circumstances which it will be one of the objects of this paper to explain.

50. If we now turn our attention to the copper plate, we shall find that the circumstances are exactly reversed; the atmosphere of the atom, D, finding less resistance in the atmosphere of E than in the atmosphere of C, by which it is propelled towards it, flows in this direction, and becomes positive towards E. The positive surface of D, now becomes the propelling force as it regards E, while the atmosphere of F forms the resisting force; but F, in its natural state is inferior to the positive force of D, and, consequently, E becomes positive towards F. The same may be said of F, and any other atoms, G, H, &c. which may follow in the copper plate, until we arrive at the surface. These actions, as we have before observed with regard to the zinc plate, take place simultaneously.

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* Plate XI. will appear with the next number.

51. Whatever, therefore, may be the depth of the two plates, provided it be not sufficient to exhaust the deranging force, the external surface of the zinc must be negative, and the surface in contact with the copper positive; while, on the contrary, the surface of the copper in contact with the zinc must be negative, and its external surface positive; and the atoms of both plates will present, alternately, positive and negative surfaces throughout the series.

52. It is evident, however, that the deranging influence of the two plates must be lessened in force as we proceed from the surfaces in contact towards the external surfaces; for they might be of such a thickness as would completely exhaust the force, and leave their distant surfaces in a natural state. The decay of force must, consequently, be gradual; B, in the zinc plate, must be less deranged than C, and A less than B; it follows, that the positive surface of B must be exposed to a greater deranging force from C, than its negative surface is from the more distant atom A, and that it will consequently be charged positively in a higher degree than it is charged negatively. The same may be said of C, and of every other atom in the zinc plate. If we turn to the copper, we shall find the circumstances similar, but reversed; the negative surface of the atom E, for instance, is exposed to the deranging influence of the atom D, which is in contact with the zinc, and its positive surface to the less powerful, because more distant, atom F; it will, therefore, be charged negatively in a higher degree than it is charged positively; and the same may be said of every other atom in the copper plate. Now, as the atoms of the zinc plate are charged positively in a higher degree than they are charged negatively, it follows, that this plate will require a greater quantity of light than naturally belongs to it; and as the atoms of the copper plate are charged negatively in a higher degree than they are charged positively, the copper plate must have a quantity of light to dispose of; the plates, then being in contact, and conductors, will arrive at a state of equilibrium with the force in action, by a transfer from one to the other (23).

53. It is not, therefore, the derangement of the atoms of the two plates which produce the transfer of light from one to the other (47), but the unequal derangement of their positive and negative surfaces; for if the atoms were charged positively in the same degree that they are charged negatively, and as they would be if there was no resistance to it, the one would exactly compensate the other. If the derangement of the atoms, without the consideration of its inequality upon their opposite surfaces, produced a greater or a less capacity for light, it ought

to be the same in both plates, and not greater in one and less in the other, as we find it; because the atoms of both plates are deranged in a similar manner.

54. It may be stated as a general law, that when two surfaces, in different electrical states, are brought into contact, there is a disposition, attended by a certain force, to transfer light from the negative to the positive surface; and that the transfer in every such case is made accordingly, unless it be opposed by an equal or a superior force.

55. The resistance to derangement, which causes the unequal charging of the positive and negative surfaces of the same atom, arises principally from the external surfaces of the plates being in contact with a body (the atmosphere) which, in consequence of its being a non-conductor, refuses to assume a corresponding state of derangement; and it requires very little penetration to discover upon looking at the figure, that if we could bring F and A into contact, the resistance arising from this cause would be completely removed by their exact correspondence with each other. But the fact, that if we bring A into communication with F, by means of a slender wire, W, supposing A and F to be parts of extended surfaces, the effect will be nearly the same as by bringing these surfaces into actual contact, is what theory could scarcely have predicted. Yet such is the case.

56. When we bring C and D into contact, there is a transfer of light from the copper to the zinc, in consequence of the resistance to derangement in the plates; and when we bring A and F into contact, or, which in effect is the same thing, when we connect the surfaces represented by A and F, by means of the wire W, there is a current from the zinc to the copper, in consequence of the removal of this resistance. It is probable, that when the resistance to derangement is removed by completing the circuit, there is a further transfer of light from the copper to the zinc; and, if so, the current which passes through W, is equal in quantity to both these transfers. Although the currents in these two cases arise from opposite causes, the one from the resistance to derangement, and the other from the removal of that resistance, they are, in both cases, in accordance with our general law (54), from a negative to a positive surface. It must be observed, however, that to render this law generally applicable, we must take into consideration the state of the surfaces as they are *induced* by the action upon each other of the *whole* of the bodies included in the circuit. In the present case, if we bring F and A into actual contact, we have only to consider the mutual action of the zinc and copper (144).

57. It has been usually supposed, that upon completing the circuit, the plates are brought into a natural state: but it will be found that, so far from this being the case, the atoms of both plates are more highly deranged, not only by the removal of resistance (55), but, in all probability, by the inductive influence of the surfaces upon each other; for when F is brought into communication with A, the positive force of the former will produce the same effect in relation to the negative force of the latter, that the positive force of C exercises upon the negative force of D, on the opposite surfaces of the plates.* When the surfaces C and D are simply brought into contact, the atoms of the plates are deranged in the same manner as the cylinder and rubber of an electrical machine, and the light is transferred from one to the other in consequence of the resistance to derangement in both cases (15); but when the circuit is completed, this resistance being removed, every atom in the two plates is charged positively in the same degree that it is charged negatively, precisely like a Leyden jar. The plates if separated when in the former state will be positively and negatively charged; while, if they are separated in the latter state, they will become neutral, because the opposite surfaces of the atoms will compensate each other.

58. If the wire W acted merely as a conductor of the electric fluid, or light, to bring the plates to a natural state, upon the same principle that we discharge a Leyden jar, the equilibrium being once established would continue; but we find that this is not the case, for upon breaking the circuit by the removal of the wire, the plates return to their previous state, and are again prepared to transmit the same quantity of light as before, upon its being replaced (60).

59. The transfer of light from the negative to the positive surface upon the first contact of the metals and upon completing the circuit, are both entirely independent of chemical action, and they form what I propose to call Galvanic currents;

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* If A and F were brought into actual contact, the equal forces on the opposite surfaces of the plates would, perhaps, neutralize each other; to prevent this, the surfaces A and F are usually connected by a conductor somewhat inferior to that by which C and D communicate with each other. In the dry galvanic pile, the surfaces of the metals are placed in contact on one side and separated by films of paper on the other; in the present case, which is intended merely to show the principle of action, I have supposed the wire to be a sufficiently inferior conductor from A to F, compared with the actual contact of C and D, to produce the same effect (108. 171).

to distinguish them from the currents produced by chemical action, which I propose to call Voltaic currents. The force by which these currents are put in motion, which I shall call the electro-motive force, increases with the increased difference of electrical force in the two surfaces brought into contact.

60. If we charge a plate of glass with common electricity, and then bring it into contact with a plate of metal, the atoms of the metal will instantly assume the same state of derangement as the glass; but if we bring a second plate of glass into contact with the charged plate, it is well known that it will not assume this state, except in a very slight degree, because the glass is a non-conductor. Let, then, this second plate of glass be coated on both sides, so as to preserve its insulation, and while it is under the inductive influence of the charged plate, let its two surfaces be brought into communication by means of a conductor, when the second plate of glass and its coatings will be charged, in an equal degree, positively and negatively upon its opposite surfaces, by the means which are usually adopted to discharge it (19). It does not follow then, because there is a current, upon completing the circuit, in the two plates of metal, that the plates must be brought to a natural state; for while the metallic surfaces are in contact their electrical forces are in action upon each other, and it is as improbable that we can discharge them under these circumstances, as that we can discharge the second plate of glass, while it remains under the inductive influence of the first, by repetition of the means we have just adopted to charge it (58).

61. There is other evidence that when the circuit is completed the plates are not in a natural state; and we may notice as an instance of it, that, although, under these circumstances all galvanic action is suspended, the plates are much more fully prepared for chemical action than when their external surfaces are unconnected; and this brings us to one of our principal objects—the nature of this preparation.

62. But, before we proceed, it is necessary to correct another very prevailing mistake. Zinc was known to be positive to copper, and as positive and negative bodies are observed to attract each other, it was very natural to infer that those elements which were separated at the zinc surface were negative, and that those which were transferred to the copper surface were positive; but, if our theory be correct, each of these metals, when plates formed of them are brought into contact, have positive and negative surfaces; and, although zinc is the positive metal, the surface which submits to chemical action is negative; while the surface of the copper, with

which the circuit is completed, and to which one of the elements of the electrolyte is transferred, is positive. (See fig. 76.) Hence we conclude, that oxygen and other elementary bodies which are deposited at the surface of the zinc, are positive; and that the class of bodies, including hydrogen, which are deposited at the surface of the copper, are negative.* This conclusion is corroborated by the fact, that bodies which are thus placed in the negative class, have higher refracting powers than those belonging to the positive class; the latter repelling the transmitted light towards the former, in consequence of the difference of force in their respective atmospheres.

63. In order that we may understand the nature of chemical action, or the means by which bodies are separated and prepared for other combinations, it is necessary that we should be acquainted with the nature of cohesion, or the means by which they were originally united. We shall have little difficulty in explaining this, because it depends upon the same electrical principles that we have already described; the only difference being, that in cohesion much higher forces are brought into action. When, for example, an atom of oxygen is brought into contact with an atom of hydrogen, the former being positive to the latter (62), the atmosphere of the oxygen flows towards the hydrogen; while the atmosphere of the hydrogen recedes from the oxygen, in the manner already described with regard to zinc and copper (49), and the oxygen, in consequence, presents a positive surface to a negative surface of the hydrogen; but the electrical forces being much more powerful than between zinc and copper, the attraction between the surfaces, under favourable circumstances, produces cohesion, and the atom of oxygen, united by its positive surface to the negative surface of the atom of hydrogen, as in fig. 77, forms a particle of water (37).

64. It will be observed that the particle of water formed by this union, presents a negative sign on the side of the oxygen and a positive sign on the side of the hydrogen; but

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* Dr Faraday has adopted the following cautious language, in speaking on this subject. "Substances are frequently spoken of as being *electro-negative* or *electro-positive*, according as they go under the supposed influence of a direct attraction to the positive or negative pole. But the terms are much too significant for the use to which I should have to put them; for though the meanings are perhaps right, they are only hypothetical, and may be wrong; and then through a very imperceptible, but still very dangerous, because continual, influence, they do great injury to science, by contracting and limiting the habitual views of those engaged in pursuing it." Researches 665.

these signs are intended to distinguish the state of the surfaces to which they are attached, as compared with the surfaces of the same atoms in contact, and not to show the state of these surfaces as compared with each other, or to other bodies to which they may be presented. But, whatever may be the arrangement of the forces upon the external surfaces of the atoms which form the particle of water, the oxygen side of it must be positive when compared with the hydrogen side, because as a particle it may be considered as turning upon its centre of gravity between the two atoms; the oxygen, therefore, in whatever way its atmosphere is arranged, will turn towards the negative surface, and the hydrogen towards the positive surface, in obedience to their collected forces respectively; whereas the atoms, when separate, turn upon their own centres and are directed agreeably to their own polarities. The distinction here made between the polarity of the particle of water, taken as the whole, and the polarity of the atoms of which it is formed, is material; because it is directed to the surface in galvanic arrangements by the former force, and, when in contact, acts upon that surface, in consequence of greater contiguity, with the electrical force of the latter.

65. Having explained, what would otherwise appear anomalous, the presentation of surfaces with negative signs to negative surfaces, and of surfaces with positive signs to positive surfaces, the actual state of the metals and fluid, in a state of voltaic combination, may be represented by fig. 78.

66. This arrangement must, at first sight, appear to be a most unfavourable state of the surfaces for the decomposition of the water by the zinc. The negative surface of the metal is in contact with the negative surface of the oxygen; and the only attraction between these surfaces appears to arise from some little difference in their negative states, which must render them positive and negative as it regards each other. This trifling force appears to have little chance of disuniting the atom of oxygen, from its atom of hydrogen preparatory to its union with the zinc, the two atoms being kept in a state of cohesion by very powerful forces, it being evident that in order to effect the exchange, the preponderance of force must be on the side of the zinc. The only way, then, that this object can be accomplished, and it may be thus attained without the slightest difficulty, is, by reversing the poles of the whole line of atoms which form the fluid particles between the two metallic surfaces.

67. "The electrical fluid, or light, is conducted from point to point, by giving the form of a current to the atmospheres of the intermediate atoms. The light which is to be trans-

mitted supplies the place of that which is pressed forward, and, consequently, it is required to be of precisely the same intensity; but when the surfaces are alternately positive and negative, this cannot be the case, and, as we have already observed, bodies thus deranged cease to be conductors" in the direction of their deranged surfaces.* (See fig. 79).

This figure represents the arrangement which the particles of water, agreeably to the explanation we have already given, (64) must necessarily take when under the influence of the polar surfaces induced by galvanic action; and, consequently, the state in which it is required to transmit a current from the zinc plate, Z, to the copper plate C. It is evident that the negative surfaces of the atoms of oxygen, O, cannot be pressed forward to supply the places of their positive surfaces; and that the latter cannot be substitutes for the negative surfaces of the atoms of hydrogen H; water, therefore, when thus arranged, is a non-conductor from Z to C, or in the direction of its deranged surfaces

68. It is necessary to recollect that the atoms of matter which form the different bodies in the circuit, figs. 78 and 79, are connected together in such a manner that no change whatever can be made in the light which forms the atmosphere of any one of these atoms, without producing a corresponding arrangement in the atmosphere of every other atom in the circuit (49). If, for instance, we produce a current of light from the copper to the zinc, by completing the circuit with the wire A B, the atoms of the wire in contact with the copper can only receive the added light by parting with an equal quantity from some other part of their surfaces, in the line of transmission, to the adjoining atoms; and as these and other succeeding atoms can only receive it upon the same condition, the instant the end B of the wire receives the light from the copper, an equal quantity appears at the end A, which is connected with the zinc; but the zinc can only receive it upon the like condition, and it is at the same instant transmitted to the negative surface of the atom of oxygen, in contact with it, which forms the commencement of the fluid part of the circuit. Now, the light upon the positive surface of this atom is more compressed, and, consequently, its atoms are nearer to each other and in a state of greater intensity (136) than on its negative surface; and we might as reasonably expect the common air of the atmosphere to pass without force into a vessel of condensed air, as that the light should readily pass from the negative surface of the atom of oxygen

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* Abstract (23.)

to its positive surface. But the oxygen can without any difficulty receive the transmitted light upon its negative surface provided it be at liberty to transfer an equal quantity from its positive surface to the negative surface of the atom of hydrogen to which it is united. The atom of hydrogen will receive it upon the like condition of transferring the same quantity from its positive surface to the atom of oxygen adjoining to it; and as similar conditions must attach to every atom of oxygen and hydrogen forming the interposed particles of water, the light must be transferred from the positive surface to the last atom of hydrogen to the surface of the copper, with which it is in contact, at the same instant that it is transmitted by the wire from its opposite surface to the zinc. The light, therefore, forms a current through the whole of the circuit, without making any alteration in the quantity permanently attached to each atom; and whatever may be its velocity or the length of the circuit, it will be in motion in every part of it at the same instant.

69. It is evident, however, that the oxygen can only receive the transmitted light from the zinc, until its negative surface becomes positive; and that if no change of arrangement were to be made in the three interposed particles of water in fig. 78, the current must then cease. But the affinities of the atoms which form these three particles are now wholly changed by the reversal of their poles; the first atom of oxygen, therefore, unites cohesively with the zinc, leaving its atom of hydrogen to unite to the second atom of oxygen: this atom, in like manner, and from the same cause, gives up its hydrogen to the third atom of oxygen, the hydrogen previously belonging to which being at the same time separated at the surface of the copper (102). (This arrangement is described in fig. 80). The oxidated zinc, then, being dissolved by the acid usually present, and the separated atom of hydrogen assuming its gaseous form, the circuit is broken at both ends of the fluid, and the plates resume their former state (58); a new arrangement is readily made (64); another particle of water supplies the place of that which has been decomposed; and the process proceeds as before.

70. The sulphuric acid, usually added to the water in voltaic arrangements, facilitates decomposition by removing the obstacles to its progress; but, as it is not itself decomposed, it cannot furnish any part of the light which forms the current, nor does it appear that it increases the force with which it circulates (89). The addition of light from this source would evidently be superfluous; if there be any lessened capacity in the acid upon entering into combination with the

zinc, the light set at liberty must take the form of caloric. These views, I believe, are supported by the experiments of Dr. Faraday and other philosophers.

71. But it will probably be asked, why is all this machinery set in motion to effect an object, which appears to be accomplished with so much ease when zinc is plunged into diluted acid? To this question we reply, that the means here described are not only the most simple, but the only means by which the different preparations can be simultaneously effected; it being necessary that the atom of oxygen should acquire a positive surface to enable it to unite with the zinc, at the same instant that it is removed from its opposite surface to disunite it from the hydrogen (102). In fact, these are the means which nature has adopted in the case alluded to; local circuits in the metal operated upon supplying the place of those which are more effectually furnished by a combination of dissimilar metals in a galvanic arrangement. (See Dr. Faraday's Researches, 947, 959).

72. Mr. Sturgeon has given a very good description of these local circuits, which he attributes to a difference in the electrical state of different parts of the same metal; and this, he justly concludes, must give to these different parts the properties of dissimilar metals.* M. de la Rive says, "the lively action of diluted sulphuric acid upon common zinc is due to a small proportion of iron in the zinc, and to the electrical currents which, taking place upon its surface, pass from the particles of zinc to the particles of iron."† It appears likely these local circuits arise in a great measure from the presence of some other metal; for the action of the acidulated water upon distilled or pure zinc, is much less considerable than upon common zinc, no doubt, in consequence of the want of this inducement to the formation of such circuits.

73. Whatever may be the circumstances which lead to the establishment of positive and negative polarities in the same piece of metal, zinc, for instance, when exposed to chemical action in dilute sulphuric, or any other acid, the circuits are formed upon the principles and in the manner we have already described (65). The oxygen enters into combination with the zinc, where the latter exposes a negative surface, and between this and a part of the same piece of metal which exhibits a positive surface, and with which the hydrogen is in contact, the poles of the atoms of the interposed particles of water are reversed; every line or curve which forms one of

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* Annals, Vol. I. page 12.

† Philosophical Magazine, Vol. II. page 282.

these circuits, giving up an atom of oxygen to the negative surface of an atom of zinc, and depositing an atom of hydrogen at the positive surface of an atom of the same piece of metal. The light which this atom of hydrogen relinquishes from its positive surface, in exchange for an equal quantity received upon its negative surface in the process of reversing its poles, is conducted *through the metal* from the positive to the negative surface before described, and from thence to the negative surface of the oxygen, to prepare it for its union with the zinc; it being absolutely essential to this union, that the oxygen, being the positive body, should present the positive surface (102). These transfers, although it is difficult to describe them otherwise than as progressive, are simultaneous.

74. The local circuits we have described are not confined to metals which are separately exposed to chemical action, but they are also formed in the zinc plates, of a battery, even when its circuit is completed, and produce the destruction of the metal without adding to the force or the quantity of the transmitted current.* The evil increases with the time the battery is in action, more particularly when solutions of metallic salts, such as sulphate of copper, are employed as the chemical menstruum; in which case the deposition of the copper upon the zinc plates forms secondary galvanic arrangements, which when carried to a certain extent must completely destroy the effect of the battery, by expanding the whole of the electro-motive force, arising from the chemical action upon the plates, in local currents. The battery, however, when its circuit is completed, is less liable to local circuits than a piece of metal unconnected with other metals; because light will always take the rout which offers the least resistance to its transmission, without reference to distance; and the circuit produced by the inductive influence of dissimilar metals always presents such a rout, unless it be obstructed by the interposition of bad conductors. Hence, when a battery is loaded with resisting decompositions, or other work, which approaches the limits of its power, the local circuits are increased; the light finding in these circuits less resistance to its progress than in the battery thus obstructed; a single non-conducting particle in any part of the circuit being fatal to its transmission.†

75. The circuits which are formed in fluids, and by means of which nature performs a great part of her most important

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* See Dr. Faraday's Researches, 996.

† See M. de la Rive's Researches, Philosophical Magazine, Vol. II. page 292.

operations, have hitherto been almost entirely neglected; I am not aware that the subject has been noticed, except in a paper of my own, "On capillary attraction and on the disposition there is in fluids to assume a globular form." Published in the fourth volume of the Records of General Science.

The fact is, that there is no such thing as an unconnected particle of fluid; every particle of every body of fluid, whether small or great, is united by means of its polar surfaces to other particles, and these to others in succession throughout the whole; and the terminating surfaces either return into each other and form a globular mass, or the nearest approach to it which circumstances will allow, or attach themselves to other bodies with which they happen to be in contact; so that in either case, no polar surface is left unconnected. In the first of these cases, the fluid when in small quantities is formed into globules, or drops; and when in larger quantities either takes the form of the containing vessel, or when this is not supplied, spreads over the ground by its gravitating force, producing evidence in the rounded form of the edges, and of every other part which is left at liberty to exhibit it, of the disposition to assume a globular form. In the second case, the terminating surfaces attach themselves to the sides of the containing vessel, and, when these are near to each other, produce the phenomena of capillary attraction; or, when at distances which allow the central parts to fall by the force of gravity, the appearance is produced of a rising of the fluid against the sides of the vessel, upon the same principle. Numerous other instances might be brought forward of this general disposition in fluids. It is seen in drops of rain, in hail, in a glass filled above its level, and under a great variety of other circumstances in water and fluids of a similar description. It is seen in the manufacturing of shot, in which the lead naturally assumes a more perfectly globular form than could be given by casting in a mould. It is seen when melted metals are poured on flat surfaces, the globular form being retained the more perfectly in proportion to the smallness of the quantity; and it is seen to great advantage in mercury, in which every variety of form is exhibited, from the perfect globe in small quantities, upon which gravitation has little or no influence, to the almost level surface, which retains only the circular form and rounded edges, under the greater influence of the same force upon larger quantities.

76. The globular form is given to fluids by two opposing forces; one of these forces disposes the particles to join their polar forces in right lines, the poles being opposite to each other upon the particles; and the other, which is the most powerful, disposes

the terminating surfaces of these lines to meet, so as to unite; the result is a circle, because in this figure every two connected particles makes the nearest approach to a right line which is consistent with the forming of a closed circuit. The force which unites the terminating surfaces, is propagated in the fluid upon the same principle that magnetic curves are formed in the air; not by any direct attraction of these surfaces for each other while they continue at any distance, but through the medium of intermediate particles of the fluid, which if they did not afterwards form part of the circle would draw them together. Precisely as the magnetic curves in the air would draw the poles of the compass needle together if it were a fluid; but which, in consequence of its being a solid, can only give it direction as a whole.

77. The subject of local circuits is well illustrated in the formation of the Lead Tree and the Arbor Dianæ. To produce the former, to which I shall confine myself, a piece of zinc is suspended in a solution of acitate of lead. Circuits are formed in the water of the solution, as we have already described with regard to zinc when plunged into diluted acid; (73) the oxygen adheres to the zinc where it presents a negative surface, the hydrogen to a part which presents a positive surface; and the current which reverses the poles passes from the hydrogen through the metal, as a conductor to the oxygen. When the oxygen has acquired its positive surface, it combines with the zinc at one end of the circuit as in the diluted acid; but instead of releasing an atom of hydrogen at the other end, the circuit is terminated by a particle of the acitate of lead,* from which the hydrogen, in contact with it, supplies itself with an equivalent for the oxygen taken from its opposite surface; leaving the lead, in its metallic state, attached by its newly acquired negative surface to a positive surface of the zinc. The positive parts of the surface of the zinc being thus occupied, the circuits are now formed between the negative surfaces of the metal and the lead already deposited upon the other parts of its surface, the atoms of which present their positive surfaces to the solution. As the operation proceeds, therefore, the metallic lead is added to that which was before deposited; an atom of oxygen being taken

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* This is a part of the arrangement necessary to the production of what are called secondary results. The hydrogen separated at the cathode, cannot take oxygen from an oxide in solution unless it be connected with it during the whole of the operation; the same preparation being required as in a particle of water, for which it is the substitute.

from the lead at one termination of the fluid part of the circuit, and added to the zinc at the other end, upon every reversal of the poles of the intermediate atoms. The oxide is, of course, separated from the negative surfaces of the atoms of the zinc by the acid set at liberty by the reduction of the lead.

(To be continued.)

LIX. *Report of the Committee appointed by the London Electrical Society to test the action of an Instrument invented by* LIEUT. R. J. MORRISON, R. N., *and denominated by him a Portable Magnetic Electrometer.*

Read 21st of April, 1838.

On Saturday, 17th February, 1838, the above instrument was submitted by the inventor to a meeting of the members of the London Electrical Society, and a description of a series of observations which had been made by some of the members of the Meteorological Society was read. The Meteorological Society has since published a report of the observations made with this instrument, wherein the deflections of the magnetic needle are considered as caused by atmospherical electric action; the easterly deflection being stated as + or positive, and the westerly as — or negative.

Many of the members of the Electrical Society, present at the above meeting, stated it as their opinion that the deflections could not be caused by electric action. A resolution, however, was proposed and carried, that the instrument be more carefully examined, and for which purpose a committee should be appointed.

Your committee proceeded at an early day to examine the instrument, and a memorandum was prepared and a copy thereof forwarded to Lieut. Morrison, suggesting that Lieut. Morrison should (previous to the committee preparing any report, or attempting any further test of the action of the instrument) vary *his* experiments. An interview took place between Lieut. Morrison and some of the members, but he declined preparing the different instruments as suggested by the committee, being perfectly satisfied as to the correctness of his own experiments as well as of the action of the instrument.

Your committee then submitted the instrument to one of the members of the society, who, after the lapse of a few days, returned it with a note of his observations thereon; this note was read to the members present at the evening meeting of 17th March. It being, however, the opinion of the majority of the members then present, that, considering the resolution passed on 17th February, the instrument ought not only to be tested by the committee, but a report describing the nature and character of the experiments should be presented to the society; and such opinion having been confirmed at the meeting of the 7th April, your committee have now in fulfilment of the duty you have imposed on them to present the following report.

REPORT.

Description of Lieut. Morrison's portable magnetic electrometer and apparatus used by the committee in testing the action thereof.

Fig. 74, plate IX, represents the instrument received from Mr. R. C. Woods, instrument maker to the Meteorological Society. 3, is an open topped bell glass which stands on a wooden base A, furnished with a compass-card. The glass is 4 inches high and 3½ inches diameter. It is not fixed to its base but merely rests on it in a groove. 2, is a brass ball surmounting a cap of the same metal, which is cemented to the neck of the bell glass. 1, is a brass rod 22 inches long, pointed at the upper extremity. Its lower extremity screws into the ball 2. 4, is the magnetic needle suspended by a gold lace thread.

Another instrument, similar to that already described, was received from Mr. Woods on the 14th April; and is that with which the deflections were observed as published in the report of the Meteorological Society. When referred to, this instrument will be called B.

Fig. 75 is a similar instrument made by Mr. E. M. Clarke; pointed wire 2 feet one inch long; glass receiver 8 inches high, 4¼ inches diameter; magnetic needle 4 inches long, suspended originally by a fine gold wire, and subsequently by a thread of silver lace 7½ inches long.

On 11th April, at 8h. A. M., the instrument A was placed on a wooden table 3½ feet long, in the centre of a lawn, far removed from all local attraction, and the needle adjusted to the magnetic meridian (Vide Note 2).

The following table will show the deflections of the instrument A on the mornings of the 11th, 12th, and during the greater part of the 13th of April; also the deflections of C during the afternoon of the latter day: as well as the deflections exhibited on the 14th, 15th, and 16th, by a zinc needle and a slip of straw; when separately suspended in instrument A, the instrument B being in action at the same time.

		h.	m.		A.	B.	C.	
11th	April	8	15	A.M.	10°E.			Clear.
12th	,,	8	0		10			
13th	,,	8	30		10			Cloudy.
	,,	10	0		Merid.			
	,,	10	10		10 E.			
	,,	1	0	P.M.	45			
	,,	1	40		45		Merid.	
	,,	3	20		45		10°E.	
	,,	3	40		25		5	
	,,	3	45		30		10	
	,,	4	50		25		10	
	,,	5	40		25		5	
	,,	6	0		25		5	
	,,	6	20		30		5	
					Zinc Needle.			
14th	,,	7	0	P.M.	20 E.	20°E.		Cloudy.
15th	,,	8	30	A.M.	60 W.	Merid.		
16th	,,	7	45		75	10 W.		
					Slip of Straw.			
	,,	11	0		40 E.	5 E.		Variable and Stormy, with Showers of Hail & Snow.
	,,	1	25	P.M.	45 W.	Merid.		
	,,	3	15		5	,,		
	,,	3	45		60	5 E.		
	,,	5	0		10 W.	Merid.		
	,,	5	25		25	5 W.		

On the 13th, at 1 P. M. finding the deflections had increased to 45° east since 10 A. M., an electroscope was placed on the table, and connexion formed by means of a copper wire from the rod of instrument A, to the plate of the electroscope; but even with the assistance of the condensing plate, the gold leaves did not diverge in the slightest manner.

A glass rod and stick of sealing wax were then successively excited with silk and flannel, and on being brought within three or four feet of the point of instrument A, the leaves of the electroscope immediately diverged, but without any apparent effect being produced on the suspended magnetic needle of A.

The connecting wire of A and the electroscope was then removed, and on the excited rod being brought even close to the point of A, there was not any deflection of the gold leaves; proving thereby that the former divergency of the gold leaves could only be caused by the connexion being made with instrument A, the point of which became electrified whenever the excited rod was brought near.

At 1h. 40m. P. M. of the same day (13th April), the instrument C was placed on the table; and as the actions of the two instruments vary very considerably, it becomes necessary to note their different forms, particularly the lengths of the thread and needle; the needle of A being one and three-quarters of an inch long, and suspended by a gold lace thread of two inches in length; while the needle of C was four inches long, and suspended by a silver lace thread of nearly eight inches in length.

On referring to the table it will be perceived that the deflection of the needle of instrument C did not exceed 10° east; but on an excited rod being brought near the point, an immediate deflection of 100° or 120° took place easterly, whether the excited rod was glass or resin; thereby proving that the deflection was due to the principles of common electricity, and not to any electro-magnetic action, as in the latter case the deflections would have been in contrary directions.

The reason for the electricity of the excited rods acting on the needle of C and not on that of A is manifest. C was suspended by a silver lace thread eight inches long, A by a gold lace thread two inches long. The difference in the length of the thread alone was favourable to great action. This increased action, however, arises principally from the length of the needle C being four inches, while that of A was only one and three quarters of an inch; consequently the great increase of mechanical power, the needle acting as a lever.

On the 14th, Mr. R. C. Woods having, at the request of one of the committee, sent another instrument B, being that used by the Meteorological Society in the observations published in their report, the needle was removed from A, and one of zinc suspended in its place: both instruments A and B were then placed on the table, and adjusted at a deflection of 20° east, being the same as A denoted previous to its magnetic needle being removed.

On the 15th, at 8h. 30m. A. M., B was at the magnetic meridian, while A deflected 60° west.

On the 16th, at 7h. 45m. A. M., B deflected 10° west, while A deflected 75° west.

The zinc needle was then removed and one of straw was substituted: in this instance, however, care was taken that the threads of both instruments were of equal lengths, as in the case of the former suspension of the zinc, the thread was of greater length.

At 11h, A. M. the straw deflected A, 40° E.; B, 5° E. On referring to the table it will be perceived that the deflections of the straw were of the most extreme description, evidently arising from the variable and windy state of the weather.

In addition to the above, both rods were at different times removed from their respective instruments; but without any effect on the deflections, which invariably remained steady.

It having been thus proved that to whatever cause the deflection of the needle might be attributed, it could not be due to any electric action, as otherwise the leaves of the electroscope would have been affected on the 13th; on the other hand it could not be due to any peculiar property in the magnetic needle, as, in that case, the strips of zinc and straw would not have been acted on from the 14th to the 17th; on the contrary, the greater deflection of the zinc and straw evidently arises from their not having had the directive power of the magnet. Again, the rod attached to each instrument could not in any way affect the action, as the deflections remained when those rods were removed. It, therefore, became evident that the cause must arise from the string, which, as has already been stated, consists of a gold lace thread, or in other words, a twisted silk thread having a metallic surface.

A wooden stool with a hole in the centre was then procured, and the instrument A placed on it; the needle was adjusted to the magnetic meridian.

A small jar of boiling water being placed under the hole, so as to permit the steam to ascend into the receiver, the needle deflected 70° west; on removing the water and placing a lighted spirit lamp in its place, the needle very shortly returned to the meridian—proving that the deflections are evidently due to the hygrometric state of the string, although in cases where the deflections are much increased and vary suddenly, the cause may arise from the wind, which from the imperfect manner in which the intrument is made, and slight weight of the needle, causes the latter to be freely and easily acted on.

Your committee regret that in the discharge of the duty you have imposed on them, they should in justice to the Society, as well as to themselves, be compelled to make any observations which may be painful to Lieut. Morrison; but the detailed account of the experiments which have been described in this report, will, they hope, convince the Society as well as Lieut. Morrison, that they entered into the inquiry with the full determination of examining the merits of an instrument, the action of which appears to have not only engaged the attention of Lieut. Morrison for some years, but also that of many scientific individuals; the utility of which "as an electrometer," had even ordinary care been taken, would have long since been set at rest, and that valuable time which the inventor has expended in noting the hourly deflections might have been profitably employed in other pursuits.

It is, however, only due to Lieut. Morrison, to state, that previous to any preparation being made by the committee for testing the instrument, one of the members applied to Lieut. Morrison to know whether it was his wish that the committee should publish a report: the following is an extract from his letter, dated Cheltenham, March 24, 1838.

"I am obliged by your letter of yesterday's date, and as you ask me whether I wish the Society to make any official report on the instrument, I brought before it on the 3d. of March,* I will tell you the state of the case. I brought the electrometer before the British Association last August, when Sir David Brewster, as Chairman of section A, requested me to make further experiments and observations, and then report them to the Association at its next meeting. It appears to me that the objects of truth and science as regards the *cause* of the deflection of the magnet would be best attained by incorporating in my report to the British Association, reports from the Meteorological and Electrical Societies; therefore, I certainly do wish the Electrical Society to make an official report, which I hope will accord with the results of the examination, which I have no doubt, from the interest taken in the instrument by M. De la Rive, Professor Addams, and others, will be made by the British Association. It seems to me that the *result of seven years' observations* by myself and others can hardly be overthrown by the hasty conclusions of any individual, after only a day or two's examination. You mistake in supposing I object to any persons offering their opinions; but I object to any opinions being considered of value, unless founded on careful observations and experiments."

Your committee, therefore, cannot hesitate in stating that Lieut. Morrison could only have been actuated by a sincere desire to have the instrument tested, and thereby carry into full effect the suggestions of Sir David Brewster, who, it appears has requested Lieut. Morrison to report at the ensuing meeting of the British Association, the rusult of any further experiments and observations he might make with the instrument.

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* This instrument was brought before the Society on the 17 February.

Your committee are aware that the notoriety which the instrument has obtained, not only in this country, but also on the Continent, (its supposed action having been referred to as a proof of the correctness of certain theories, some of which are quite at variance with those laws hitherto admitted as axioms in physical science) may have induced many members to vote for the resolution of the 17th February, and your committee have, consequently, been induced to carry on the series of experiments herein described. Your committee, however, in conclusion, suggest, that when instruments are brought before the Society, previous to any committee being appointed to test the same, the inventors should be questioned as to whether they had taken at least some ordinary measures to prove the nature and character of the action; much time would by this course be saved, and those members who may be willing to undertake the task would be enabled to employ their time profitably to the Society and the science, whose interest it is their peculiar province to uphold.

By order of Committee,
J. V. MOORE,
Assistant Secretary,
London Electrical Society.

London, April 19, 1838.

(NOTE I.)

47, Hatton Garden, March 26, 1838.

Sir,

I have just received the acknowledgment of the receipt of Lieut. Morrison's electrometer, and beg to state that the needle is suspended in the same manner as I received it from Lieut Morrison. The original thread is, however, in the possession of Mr. Bachhoffner; but the one by which the needle is now suspended, and the spare thread accompanying it, is of the same description as the one used by me in the thirty-seven hourly observations, at the vernal equinox, published in the Meteorological Society's Report.

I am, &c.
(Signed) R. C. WOODS.

To Mr. J. V. Moore, Assistant Secretary,
London Electrical Society,
Royal Gallery of Practical Science.

P. S. I received three of these instruments from Lieut. Morrison: the one sent is the third. They were made at Bath, by whom I know not.

(NOTE II.)

From the mode of suspension, there requires another needle to adjust that of the instrument to the magnetic meridian. A needle with an agate cap, suspended on a fine steel point, is placed on the table, and a line is drawn on the magnetic meridian. The compass card and needle of Lieut. Morrison's instrument is then placed on the table and adjusted by turning the instrument with the hand to this line.

LX. *Extract of a Letter from* Mr. J. C. NESBIT *to the Editor.*

Sidney Street, March 22, 1838.

Dear Sir,

By only getting the March number to-day I shall not be able to make the experiments you desired in time for the April number; but I may here observe, that the shocks from my magnetic coil machine are less in power when the wheel is in very rapid motion; but that even though the connexion is broken upwards of 3000 times in one minute, the power of the machine is not destroyed, but continues to give shocks at every break, though not of such strength as when the wheel is rotated at a medium speed.

The coils of my magnetic machine are made of copper wire covered with cotton, as I can get it covered very cheaply in Manchester; but, to many persons in the country, my *machine* for covering wire with sealing wax or other resinous matter, may be of value, as it will enable them to cover wire for themselves at a very trifling expense.

The following is the description of the method which I use. Construct a vessel in the shape of a segment of a cylinder; let the distance, A B, fig. 73, Plate IX., be about fourteen inches, and the versed sine, C *c*, about five inches, the breadth two inches. At B is fixed a small deep grooved pulley, and at *c* is another about one and a half inch in diameter, also deeply grooved: at *a* is soldered a stout wire, bent as in the figure. *t* is a small cylinder of brass, with a hole through it in the direction of its axis, a little larger than the size of the wire intended to be covered; the diameter of the aperture may vary according as we may wish to give a thick or a thin covering to the wire. Several cylinders ought to be made of different bores, to accommodate wire of different sizes. The cylinders have small steel centres as represented at *t*, which fit severally into a steel fork or spring as at P. The bottom part of the steel spring fits into a tube at the end of E, which is a channel to convey back into the vessel the redundant resin.

The cylinder *t*, therefore, having motion in every direction, can accommodate itself to the wire. It will, however, always be best to draw the wire from the wax in a line in which E and *a* coincide. Every thing being now arranged, the wire to be covered is passed over the pulley at B, under *c*, over *a*, and through the brass cylinder at E. The brass cylinder must be heated by means of a lamp, in order to prevent it solidifying the wax. The vessel is now to be partially filled with melted sealing wax or resin, and the wire must be drawn through at a pretty quick, though regular, speed. The wax may be kept melted by a lamp placed underneath the vessel. If the wax get solid at E, it must be melted by means of the lamp. With an apparatus less perfect than this, I have covered 2500 feet of thin wire in half an hour.

The resin which I use is made by melting equal parts of shell lac and Venice turpentine together in a pipkin, taking care to melt the Venice turpentine before putting in the shell lac, which must be done gradually. If the wax should be found too brittle, it may be brought to a proper consistence by adding a little spirits of turpentine. A few trials will enable a person to judge of the right consistence of the wax.

Hoping this communication may benefit some of your numerous readers,

I remain, my dear Sir, yours, most truly,
J. C. NESBIT.

To William Sturgeon, Esq.

LXI. *London Electrical Society.*

In our report of the meeting of this society, on the 17th of February (Vol II. page 227), we omitted to state that a resolution was passed, requesting the committee to test the action of the instrument exhibited to the meeting by Lieut. Morrison. At the meeting of the 17th of March, the chairman read a communication the committee had received from Mr. Bachhoffner, one of the members of the society, describing the nature of the observations he had made with the instrument; stating it as his opinion that, to whatever cause the deflection of the magnetic needle might be owing, it could not be attributed to atmospheric electrical action. At the close of the meeting the following notice of a resolution to be proposed at the ensuing meeting was entered on the minutes.

"That the instrument exhibited to this society on the 17th of February, by Lieut. Morrison, and denominated by him a portable magnetic electrometer, be examined by the committee, and *a report thereon submitted to the society.*"

Saturday, 7th March.—Afer the minutes of the preceding meeting had been confirmed, and the names of new members announced, the preceding resolution was proposed and carried.

A paper on the principle and action of lightning conductors was then read by Mr. Sturgeon.

The author considers that however perfectly lightning conductors, as now generally used, are constructed, they will not prevent, but on the contrary, always be the means of producing the lateral discharge, or rather the returning stroke as denominated by Viscount Mahon: following out those theoretical principles developed in the papers which the author had previously read to the society, he states, that if a circle of metal were the channel of an electric discharge, the disturbing tendency would be manifested to the greatest extent outwards, because the repulsions inwards would counterbalance each other, and as the lateral discharge is a mere consequence of unrestrained repulsion, no lateral discharge could possibly take place in the interior of a hollow cylinder. The effects due to a cylindric conductor are not limited to that figure, but are also demonstrable in rectangular ones, or in any other form whatever.

Upon these principles the author considers that powder magazines, whether on shore or on board of ships of war, would be completely protected from lightning if lined throughout with sheet copper, and that copper in good metallic connexion with the ground or the water. Nothing within could possibly suffer from a lateral discharge, although the surrounding metal became the channel for the heaviest flashes of lightning. The copper lining would, moreover, be a perfect safeguard from the primitive or original flash, or discharge from a cloud. The importance of the application of this plan of security from the lateral discharge to powder magazines on board vessels or on shore was pointed out, and the estimate of the probable expense was also laid before the meeting.

The author is also of opinion that the same plan might be applied to the protection of single rooms in buildings. Persons sitting in a room completely lined with sheet copper, need be in no fear of injury from lightning, although many flashes might be discharged through the metal. The elegance of the room would not suffer by such protection, because it might be placed under the paper hangings or tapestry, and completely concealed from view.

Saturday April 21. The Report of the Committee appointed (agreeably to the resolutions of the 7th. instant) to examine the action of an instrument invented by Lieut. Morrison, R. N. and denominated by him a Pocket Magnet Electrometer, was read. After the reading of the report, which is inserted in the present number of the Annals, page 375, a discussion took place, of which the following is a brief outline.—

Mr. R. C. Woods (who was present by invitation of the Committee) did not consider that the experiments as stated in the report (several of which were repeated at the meeting) as sufficiently conclusive to warrant the Society in receiving the report. Lieut. Morrison had carried on a series of experiments with the instrument for many years. Many of these Mr. Woods had himself confirmed; the Meteorological Sociey had published observations made with the instrument; it had been noticed on the Continent; as also at the last meeting of the British Association at Liverpool, when Sir D. Brewster requested Lieut. Morrison would make a further report at the ensuing meeting of the Association. Mr. Woods therefore hoped the Electrical Society would hesitate before they sanctioned a report founded on such experiments as had been explained this evening.

Mr. Sturgeon stated that the report had evidently been prepared with great care and attention; that the experiments were well selected; and were such as would be perfectly conclusive and satisfactory to any person claiming even a moderate degree of knowledge of electricity and electro-magnetism. Mr. Woods had laid much stress on the *great number* of observations which had been made with the instrument, both by the inventor and himself. He (Mr. S.), however, could not see very distinctly, that the instrument was much better on that account; he would rather have heard some explanation of the *principles* upon which the instrument acted; or at least, by what criterion the *quality* of those observations was ascertained: for, as regards real importance, he should always consider that there is a very great difference between the *quantity* and the *quality* of both observations and experiments. His surprise was, not only that the inventor should ever consider the instrument as acting under the influence of atmospheric electricity; for it could not claim the name of even an electroscope, much less the dignity of an eletrometer; but that an instrument constructed as this is, should have obtained any notice from scientific individuals, and even scientific associations. He had no doubt now, however, that, like himself, they had all been deceived in the character of the *main spring* of this celebrated instrument; and that they had no idea whatever of its being a *twisted string*. For his own part he could not have thought it possible that any man could have hit upon so singular a contrivance for the suspension of a magnetic needle. The instrument is obviously an hygrometer. Mr. S. had long seen the necessity of an Electrical Society, and if any thing could be wanting to prove the correctness of his views, the occurrences relative to this instrument were alone sufficient; for here we have an instrument, which has not only occupied the attention of the inventor for a series of years, but a metropolitan scientific society has actually, by including its supposed indications with those of the barometer, thermometer, &c. in their meteorological report, given a currency to an instrument which, in all probability, it would never otherwise have obtained. The Royal Academy of Science at Brussels has since described, and referred to it in one of their reports; and thus an instrument, insignificant in itself, has become important in the estimation of the world by the sanction it has obtained from these learned bodies.

Mr. Leithead, Hon. Sec., (who had, by indisposition, been prevented from attending any of the meetings of the committee) agreed with Mr. Woods, that the report was not sufficiently conclusive to warrant the society in receiving it. Although atmospheric electricity might not have affected the gold leaf electroscope, it was not any proof that it did not affect the magnetic needle. The Aurora Borealis affected the needle while it had not any effect on the gold leaf electroscope. Mr. L. added that he hoped on a future day to lay before the society a series of experiments and observations on this subject, which he was at present making but which his recent indisposition had delayed.

Mr. Dixon stated that he had been present when many of the experiments were made, and that nothing could have exceeded the anxiety of the committee, that the report was in every way conclusive: he was fully aware it would have been a much more pleasing duty for the society to have reported favourably of such an instrument: for if one instrument more than another was wanting, it was a perfect electrometer. Mr. Dixon concluded by proposing that the report be received and confirmed, that the thanks of the society be given to the committee, and that the same be entered on the minutes of the meeting. The foregoing resolution having been carried, it was resolved, that a copy of the report be forwarded to Lieut. Morrison, to the Meteorological Society, and to such individuals and scientific bodies, as the committee may deem advisable.

LXII. *Proceedings of the Royal Society.*

February 15, 1838.

DAVIES GILBERT, Esq. Vice-President, in the Chair.

A paper was in part read, entitled "Experimental Researches in Electricity," Twelfth Series, by Michael Faraday, Esq., D.C.L., F.R.S., &c.

February 22, 1838.

FRANCIS BAILY, Esq., V. P. and Treas., in the Chair.

William Thomas Denison, Esq., R. E., and Joseph Locke, Esq., were elected Fellows of the Society.

The reading of a paper, entitled "Experimental Researches in Electricity," Twelfth Series, by M. Faraday, Esq., D.C.L., F.R.S., was resumed.

March 1, 1838.

The Right Honourable the EARL of BURLINGTON, Vice-President, in the Chair.

Alexander Wilson, Esq., was elected a Fellow of the Society.

The reading of a paper, entitled "Experimental Researches in Electricity," Twelfth Series, by Michael Faraday, Esq., D.C.L., F.R.S., &c., was resumed and concluded.

Experimental Researches in Electricity: Twelfth Series. By Michael Faraday, Esq., D.C.L., F.R.S., Fullerian Professor of Physiology in the Royal Institution of Great Britain.

The object of the present series of researches is to examine how far the principal general facts in electricity are explicable on the theory adopted by the author, and detailed in his last memoir, relative to the nature of inductive action. The operation of a body charged with electricity, of either the positive or negative kind, on other bodies in its vicinity, as long as it retains the whole of its charge, may be regarded as *simple induction*, in contradistinction to the effects which follow the destruction of this statical equilibrium, and imply a transit of the electrical forces from the charged body to those at a distance, and which comprehend the phenomena of the *electric discharge*. Having considered, in the preceding paper, the process by which the former condition is established, and which consists in the successive polarization of series of contiguous particles of the interposed insulating dielectric; the author here proceeds to trace the process, which taking place consequently on simple induction, terminates in that sudden and often violent interchange of electric forces constituting *disruption*, or the electric discharge. He investigates, by the application of his theory, the gradual steps of transition which may be traced between perfect insulation on the one hand, and perfect conduction on the other, derived from the varied degrees of specific electric relations subsisting among the particular substances interposed in the circuit: and from this train of reasoning he deduces the conclusion that *induction* and *conduction* not only depend essentially on the same principles, but that they may be regarded as being of the same nature, and as differing merely in degree.

The fact ascertained by Professor Wheatstone, that electric conduction, even in the most perfect conductors, as the metals, requires for its completion a certain appreciable time, is adduced in corroboration of these views; for any retardation, however small, in the transmission of electric forces can result only from induction; the degree of retardation, and, of course, the time employed, being proportional to the capacity of the particles of the conducting body for retaining a given intensity of inductive charge. The more perfect insulators, as lac, glass, and sulphur, are capable of retaining electricity of high intensity; while on the contrary, the metals and other excellent conductors, possess no power of retention when the intensity of the charge exceeds the lowest degrees. It would appear, however, that gases possess a power of perfect insulation, and that the effects generally referred to their capacity of conduction, are only the results of the carrying power of the charged particles either of the gas, or of minute particles of dust which may be present in them; and they perhaps owe their character of perfect insulators to their peculiar physical state, and to the condition of separation under which their particles are placed. The changes produced by heat on the conducting power of different bodies is not uniform; for in some, as sulphuret of silver and fluoride of lead, it is increased; while in others, as in the metals and the gases, it is diminished by an augmentation of temperature.

One peculiar form of electric discharges is that which attends *electrolyzation*, an effect involving previous induction; which induction has been shown to take place throughout linear series of polarized particles, in perfect accordance with the views entertained by the author of the general theory of inductive action. The peculiar feature of this mode of discharge, however, is in its consisting, not in a mere interchange of electric forces at the adjacent poles of contiguous particles, but in their actual separation into their two constituent particles; those of each kind travelling onwards in contrary directions, and retaining the whole amount of the force they had acquired during the previous polarization. The lines of inductive action which occur in fluid electrolytes are exemplified by employing for that purpose clean rectified oil of turpentine, containing a few minute fibres of very clean dry white silk; for when the voltaic circuit is made by the introduction into the fluid of wires, passing through glass tubes, the particles of silk are seen to gather together from all parts, and to form bands of considerable tenacity, extending between the ends of the wires, and presenting a striking analogy to the arrangement and adhesion of the particles of iron filings between the poles of a horse-shoe magnet.

The fact that water acquires greater power of electrolytic induction by the addition of sulphuric acid, which not being itself decomposed, can act only by giving increased facility of conduction, is adduced as confirming the views of the author.

The phenomena of the disruptive electric discharge are next examined with reference to this theory: the series of inductive actions which invariably precede it are minutely investigated: and reference is made to the accurate results obtained by Mr. Harris, as to the law of relation between the intensity of a charge, and the distance at which a discharge takes place through the air.

The theory of Biot and others, which ascribes the retention of a charge of electricity in an insulated body to the pressure of the surrounding atmosphere, is shown to be inconsistent with various phenomena, which are readily explained by the theory adopted by the author.

The author then enters into an enquiry relative to the specific conducting capacities of different dielectrics.

With a view of determining the degrees of resistance to the transit of electricity excited by different kinds of gases, he constructed an apparatus, in which an electric discharge could be made along either of two separate channels; the one passing through a receiver filled with the gas, which was to be the subject of experiment, and the other having atmospheric air interposed. By varying the length of the passage through the latter, until it was found that the discharge occurred with equal facility through either channel, a measure was afforded of the relative resistances in those two lines of transit, and a determination consequently obtainded of the specific insulating power of the gas employed.

The circumstances attending the diversified forms of the disruptive discharge, such as the vivid flash or spark, the brush or pencil of light, and the lucid point or star, which severally represent different conditions of the sudden transit of electrical forces through an intervening dielectric, are minutely investigated in their various modifications. The spark is the discharge, or reduction of the polarized inductive state of many dielectric particles, by the particular action of a few of those particles occupying but a small and limited space, leaving the others to return to their original or normal condition in the inverse order in which they had become polarized: and its path is determined by the superior tension which certain particles have acquired, compared with others, and along which the action is accordingly conducted in preference to other lines of transit. The variety in the appearance of the electric spark taken in different gases may be ascribed partly to different degrees of heat evolved, but chiefly to specific properties of the gas itself with relation to the electric forces. These properties appear also to give occasion to diversities in the form of the pencil or brush, which takes place when the discharge is incomplete, and is repeated at short intervals, according to the shape of the conductor on either side, and according to the species of electricity conveyed. The diverging, converging, bent, and ramified lines presented in these different forms of electric discharge, strikingly illustrate the deflexions and curvilinear courses taken by the inductive actions which precede the disruption; these lines being not unlike the magnetic curves in which iron filings arrange themselves when under the action of opposite magnetic polarities.

March 8, 1838.

FRANCIS BAILY, Esq., V. P. and Treas., in the Chair.

Colonel Andrew Leith Hay, K. H., who had at the last Anniversary ceased to be a Fellow from the non-payment of his annual contribution, was at this meeting re-elected by ballot into the Society.

A paper was read, entitled "Proposal for a new method of determining the Longitude, by an absolute Altitude of the Moon," by John Christian Bowring, Esq. Communicated by John George Children, Esq., F. R. S.

The method employed by the author for determining the longitude by the observation of an absolute altitude of the moon, was proposed, many years ago, by Pingré and Lemmonier; and the principal difficulty which stood in the way of its adoption, was its requiring the exact determination of the moon's declination reduced to the place of observation. This difficulty the author professes to have removed by supposing two meridians for which the altitudes are to be calculated: and the only remaining requisite is the accurate determination of the latitude, which presents no great difficulty, either on land or at sea. Examples are given of the practical working of this method; showing that if the latitude of a place of observation be obtained within a few seconds, the longitude will be found by means of a single observation of the altitude of the moon.

A paper was also read, entitled, An Inquiry into a new Theory of earthy Bases of Vegetable Tissues," by the Rev. J. B. Reade, M. A., F. R. S.

The author, after briefly noticing the results of some of his experiments described in two papers which appeared in the Philosophical Magazine for July and November, 1837, and also those of Mr. Robert Rigg in a paper read to the Royal Society, next adverts to the theory of M. Raspail, detailed in his *Tableau Synoptique*, and *Nouveau Système de Chimie*. In opposition to some of the views entertained by the latter, he finds that in the bark of the bamboo and the epidermis of straw the silica incrusting these tissues is not crystallized, but, on the contrary, exhibits, both before and after incineration, the most beautiful and elaborate organization, consisting of an arranged series of cells and tubes, and differing in its character in different species of the same tribe, and in different parts of the same plant.

The observations of Mr. Golding Bird, contained in the 14th number of the Magazine of Natural History, new series, are then referred to; and the author states in confirmation, that, by employing caustic potash, the siliceous columns may be removed from the leaf of a stalk of wheat: while the spiral vessels and ducts, which form the principal ribs of the leaf, as well as the apparently metallic cups which are arranged on its surface, remain undisturbed. He proposes, therefore, to substitute, in the description of vegetable tissues, the term *skeleton*, instead of that of *bases*, whether saline or siliceous, of those tissues.

March 15, 1838.

FRANCIS BAILY, Esq., V. P. and Treas., in the Chair.

Captain Thomas Best Jervis, E. I. C. S., and Travers Twiss, Esq., were elected Fellows of the Society.

The reading of a paper, entitled, "Experimental Researches in Electricity," Thirteenth Series, by Michael Faraday, Esq., D. C. L., F. R. S., &c., was commenced.

March 22, 1838.

FRANCIS BAILY, Esq., V. P. and Treas., in the Chair.

A paper was read, entitled, Description of a new Tide-Guage, constructed by T. G. Bunt, and erected on the Eastern bank of the River Avon, in front of the Hotwell House, Bristol, in 1837." Communicated by the Rev. William Whewell, M. A., F. R. S.

The principal parts of the machine here described, are an eight-day clock, which turns a vertical cylinder, revolving once in twenty-four hours; a wheel, to which an alternate motion is communicated by a float rising and falling with the tide, and connected by a wire with the wheel which is kept constantly strained by a counterpoise; and a small drum on the same axis with the wheel, which by a suspending wire communicates 1-18th of the vertical motion of the float to a bar carrying a pencil which marks a curve on the cylinder, or on a sheet of paper wrapped round it, exhibiting the rise and fall of the tide at each moment of time. The details of the mechanism, illustrated by drawings, occupy the whole of this paper.

A paper was also read, entitled, "On the Régar or Black Cotton Soil of India," by Capt. Newbold, Aid-de-Camp to Brigadier-General Wilson. Communicated by S. H. Christie, Esq., M. A., Sec. R. S.

The author states that the Régar of India is found, by chemical analysis, to consist of silica, in a minute state of division, together with lime, alumina, oxide of iron, and minute portions of vegetable and animal *débris*. Hence it is usually considered as having been formed by the disintegration of trap rocks; the author, however, after examining its numerous trap dykes traversing the formation of the ceded districts, which he found invariably to decompose into a ferruginous red soil, perfectly distinct from the stratum of black régar through which the trap protrudes, was led to regard this opinion on its origin as erroneous: and from the circumstances of its forming an extensive stratum of soil covering a large portion of the peninsula of India, he believes it to be a sedimentary deposit from waters in a state of repose.

Specimens of basaltic trap and of the Régar soil were transmitted to the society by the author, for the purpose of analysis.

The reading of a paper, entitled, "Experimental Researches in Electricity," Thirteenth Series, by Michael Faraday, Esq. D. C. L., F. R. S. &c., was resumed but not concluded.

March 29, 1838.

JOHN GEORGE CHILDREN, Esq., V.P., in the Chair.

Simon MacGillivray, Esq., was elected a Fellow of the Society.

The reading of a paper, entitled, "Experimental Researches in Electricity." Thirteenth Series, by Michael Faraday, Esq. D. C. L., F. R. S., was resumed but not concluded.

April 5, 1838.

FRANCIS BAILY, Esq., V.P. and Treas., in the Chair.

John Hardwick, John Macneill, and Edward William Tuson, Esqs., were elected Fellows of the Society.

The reading of a paper, entitled, "Experimental Researches in Electricity," Thirteenth Series, by Michael Faraday, Esq., D.C.L., F.R.S., was resumed and concluded.

The author, in this paper, pursues the inquiry into the general differences observable in the luminous phenomena of the electric discharge, according as they proceed from bodies in the positive or the negative states, with a view to discover the cause of those differences. For the convenience of description he employs the term *inductric*, to designate those bodies from which the induction originates, and *inducteous* to denote those whose electric state is disturbed by this inductive action. He finds that an electric spark, passing from a small ball, rendered positively *inducteous*, to another ball of larger diameter, is considerably longer than when the same ball is rendered positively *inductric;* and that a similar difference, though to a less extent, is observable, when the smaller ball is rendered negative. The smaller ball, rendered positive, gives also a much longer spark than when it is rendered negative; in which latter case, however, it affords, at equal distances, a luminous brush of greater size, and gives it much more readily than when positive. In order to ascertain the relative degrees of charge which the balls acquire before the occurrence of the discharge, the author employed an apparatus attached to the insulated conductor of the electrical machine, and also to the conductor connected with the discharging train, and consequently uninsulated, consisting, on each side, of a rod branching out in the form of a fork, and terminating at one of its extremities in a large ball, and at the other in a small one; the position of the forks being capable of adjustment, so that the large ball of each rod might be brought exactly opposite to the small one of the other: and the distances between each pair admitted of being regulated at pleasure, until the discharges through each interval were rendered apparently equal to one another. From numerous experiments made with this instrument, the author concludes that when two conducting surfaces of small but equal size, are placed in air, and electrified, the one positively and the other negatively, a discharge takes place at a lower tension from the latter than from the former; but that, when a discharge does occur, a greater quantity of electricity passes at each discharge from the positive, than from the negative surface. Experiments of a similar nature were made in gases of different kinds, by enclosing them in an apparatus constructed on the same plan as the former one, but capable of acting in a receiver, from which the air could be exhausted, and the particular gas, whose powers in modifying the electric discharges were to be ascertained, could be introduced in its place. The results of various trials are given in a table, from which it appears that different gases restrain the discharge in very different degrees. The discharge from the small ball, through nitrogen and hydrogen gases, most readily takes place when the charge is positive: and through oxygen, carbonic acid, and coal gas, when it is negative.

The author next directs his attention to the peculiar luminous phenomena attending the disruptive electrical discharge, which he terms *a glow*, and which appears to depend on a quick, and almost instantaneous charge given to the air in the immediate vicinity, and in contact with the charged conductor; and he enters into a detailed account of the circumstances by which it is influenced, and its production favoured; such as the diminution of the charging surface, increase in the power of the machine, rarefaction of the surrounding air, and the particular species of electricity concerned. The relations which the glow, the brush, and the spark bear to one another, as well as the steps of transition between each are minutely investigated; and the conclusion is deduced that the glow is in its nature exactly the same as the luminous part of a brush or ramification, namely, a charge of air; the only difference being that the glow has a continuous appearance from the constant renewal of the same action in the same place, whereas the ramification is occasioned by a momentary and independent action of the same kind. The disruptive discharge may take place at degrees of tension so low as not to give rise to any luminous appearance; so that a dark space may intervene in the line of actual discharge, as is frequently observable between the brush on one side, and the glow on the other. Thus it is inferred that electric light is merely a consequence of the quantity of electricity which, after a discharge has commenced, flows and converges towards the spot where it finds the readiest passage: and these conclusions are further confirmed by the phenomena which takes place in other gases, besides atmospheric air, and which are specifically detailed by the author.

The last kind of discharge which is here considered is the *convective* or *carrying discharge*, namely, that effected by the translation of charged particles from one place to another. The phenomena attending this mode of transference are examined under the various aspects as they occur in air, in liquids of various kinds, in flame, and as they are exhibited in the case of particles of dust, which perform the office of carriers of the electricity; and also in that of solids terminated by liquids. Thus all these apparently isolated phenomena comprised under the heads of the electric currents which characterize electrolyzation, of transference through dielectrics by disruptive discharges of various kinds, or by the actual motion of charged particles, and of conduction through conductors of various degrees of power, are assimilated to one another by their being shown to be essentially the result of actions of contiguous particles of matter assuming particular states of polarization.

The author lastly considers electric currents, not only in their effects on the bodies they traverse, but also in their collateral influences as producing inductive and magnetic phenomena. The analogies, which connect electrolytic discharge with that by conduction, are pointed out, as tending to show that they are essentially the same in kind, and that when producing different kinds of motion in the particles of matter, their mode of operation may be regarded as identical. An attempt is made to connect with these views the lateral or transverse actions of currents, which are most distinctly manifested in their magnetic effects; these effects being produced equally by the disruptive, the conductive, and the electrolytic discharges, and probably depending on the transverse condition of the lines of ordinary induction. This transverse power has the character of polarity impressed upon it, and, in its simplest form, appears as attractive or repulsive, according as the currents themselves are in the same, or in opposite directions. In the current and in the magnet it assumes the condition of tangential force; and in magnets and their particles it produces poles.

The author announces that he intends shortly to develop, in another series of these researches, some further views which he entertains concerning the nature of electric forces and electric excitation in connexion with the theory he has here advanced.

The Society then adjourned over the Easter Recess to meet again on the 26th instant.

LXIII. MISCELLANEOUS ARTICLES.

In a note at bottom of page 54, of the present volume, I have stated that a specimen of clay presented to me by Mr. R. W. Fox, had been placed in a voltaic circle, for the purpose of repeating the experiments of that gentlemen on the lamination of clay by electricity. I now beg to state that during the hard frost the liquid and clay became frozen into one solid mass: and whilst dissolving again before the fire, the clay wall melted down and mixed with the liquid. This happened twice at intervals of about a month from each other. The experiment has consequently been interrupted. The clay was, however, recovered from the water, by evaporating the latter, and the experiment was resumed on the 3d. of the present month (April).

Having no copper ore similar to that used by Mr. Fox, I have employed copper and zinc for the voltaic pair: but, for fear of not being successful with the materials I was in possession of, I wrote to Mr. Fox requesting him to furnish me with a few pieces of copper ore, and a piece of clay, similar to those he employed in his experiments. I have now great pleasure and satisfaction in having this opportunity of publicly acknowledging that my request has been answered in the most handsome and liberal manner: and that, in consequence of the supply of copper ore and clay which I have received, Mr. Fox has favoured me with every facility for repeating his very interesting experiments.

The experiments are intended to commence on Tuesday the 1st of May. Every particular of their commencement will be described in the June number of the "Annals."

W. STURGEON.

To the Editor of the Annals of Electricity, &c.

Sir,

The enormous price charged by the opticians for covered copper wire, and a desire to diminish as much as possible the expense of electro-magnetic studies, induces me to inform your readers how they may get it cheaper.

Those who cover bonnet wire charge about 6*d.* *per pound*: it is only necessary then to discover a wire coverer, and supply him with the copper wire, as, thus, it will not cost one third the opticians' charge. A man of this description, whose name is Green, and who lives near the western end of Quaker Street, Spitalfields, covered lately 2½ pounds of copper wire with silk, for 2*s.* 6*d.* The first cost of the wire, which was No. 16, was 1*s.* 3*d.* per pound; thus I had 2½ pounds of well covered wire for 5*s.* 7½*d.* Smaller wire in proportion. When covered with cotton it is about 4*d.* per pound less. A full description of the apparatus by which wires are covered may be seen in the Mechanics' Magazine, No. 717.

With best wishes for the continued success of your excellent Annals,

I remain,
Yours respectfully,
G. FRANCIS.

55, *Great Prescott Street.*

A mode of analysing German Silver. By JAS. C. BOOTH.

As the employment of this interesting compound is daily becoming more general, it becomes a point of some importance to the manufacturer to ascertain with some accuracy the composition of those kinds in the market, which are adjudged to possess superior qualities. For this purpose I have contrived a method of analysing them, which may be successfully practised by any one who possesses a little chemical knowledge. A small piece of about 20 grains is dissolved in nitro-muriatic acid with the assistance of a gentle heat, by which means the metals will be converted into chlorides. If the solution be filtered through a small paper filter, and a white powder remain after washing with water, it is the chloride of silver, the presence of which metal in the compound is accidental and scarcely appreciable. The acidulated solution is then treated by sulphuretted hydrogen, which separates copper and a little arsenic. The sulphuret of copper is collected on a filter, treated with nitric acid in a gentle heat, till the sulphur appears whitish, then filtered, brought to boiling, precipitated with caustic potassa, filtered and weighed. 100 parts of this precipitate contains 79.83 of metallic copper. To the solution after filtering off the sulphuret of copper, a little nitric acid is added, and the whole heated in order to convert the protoxide into the peroxide of iron. Muriate of ammonia is then added to the same and a small excess of ammonia, which precipitates only the peroxide of iron. This may be collected on a filter and weighed, 100 parts of it contain 69.34 of metallic iron. The solution is now to be treated with carbonate of soda and evaporated to dryness; the dry mass is treated with hot water, and the residue washed and dried. This powder, consisting of carbonate of zinc and nickel, is mixed with half its weight of saltpetre and ignited until the whole is nearly dry. It is transferred to a filter after being powdered in a small mortar, and is then washed two or three times with pure, but dilute, nitric acid, which dissolves the oxide of zinc, and leaves the *peroxide* of nickel. To the zinc solution carbonate of soda is added, the whole evaporated to dryness, treated with hot water, and the remainder after being dried and ignited is weighed, 100 parts contain 80.13 metallic zinc. The peroxide of nickel is dissolved in hydro-chloric acid, precipitated by caustic potassa, filtered off and weighed, 100 parts of it contain 78.71 metallic nickel.

The separation of nickel and zinc is ever attended with difficulty and some uncertainty, but it is rendered much more simple by the method which I propose, and which is not more inaccurate than others in use. Before weighing any of the above oxides, it is decidedly preferable to burn the filter after shaking off as much of the substance as possible into a platinum crucible, to add the ashes, and then subtract their weight from that of the oxide.

Journal of the Franklin Institute.

Analysis of Solar light.

Within a few days past, notices have been circulated in the public prints, that Melloni had succeeded in depriving the sun's rays of all their heat, by transmitting them through certain media, consisting of water and coloured glasses; and also, that Mrs. Somerville, by means of a screen of plate green glass, had abstracted from them, that property by which they darken the chloride of silver, and effect chemical changes.

Whilst these results have been obtained in Europe, experiments of a like character have been carried on in Virginia, the event of which is of far more interest to chemists, the effects being equally as certain, and the means being in the hands of every experimenter. Dr. Draper, professor of chemistry in Hampden Sidney College, found during the last year, that there are several solutions, which are transparent as respects the sun's light, yet opaque to his calorific ray, and others which are transparent both to his light and heat, but opaque to the chemical ray; for it does not follow, that a body transparent to light should be transparent to heat or the chemical rays. A solution of sulphate of copper and ammonia, and a decoction of tannin are both transparent to the light of the sun, yet they are nearly opaque to his heat. Nor is this condition of things at all regulated by colour; the first mentioned of those substances which is blue, the second which is brown, and the sulphocyanate of iron which is red, the chloride of chromium which is green, the muriate of cobalt which is pink, and the bichromate of potassa which is orange, though they are all when in solution transparent to the rays of light, yet are either opaque or only translucent to the rays of heat. It has been found more recently, that solutions which are perfectly colourless and clear as water, exercise very different functions on the rays of heat, and though in an examination of upwards of two hundred and seventy such solutions, none have yet been found which are absolutely opaque to the rays of heat, there are some which approach that condition. Vegetable solutions exercise a similar influence. Turnsole dissolved in water, when the thickness is about a quarter of an inch, permits only about four rays of heat, out of every hundred which falls upon it, to pass through: this is a blue solution; a decoction of Brazil wood which is red, a decoction of Logwood in alum which is purple, and tincture of turmeric which is yellow, have the same effect.

A solution of the chromate of potassa is nearly opaque to the chemical ray, but is transparent to the ray of light, and more than semi-transparent to the ray of heat; the bichromate of potassa seems to be absolutely opaque to the chemical ray, for a beam of light three inches in diameter, conveyed to a focus by a convex lens, after traversing such a solution one-fourth of an inch thick, did not blacken chloride of silver in an exposure of fifteen minutes. All the vegetable solutions above named are likewise nearly opaque; but a solution of the sulphate of copper and ammonia, when in a mass thick enough to stop almost all the rays of light, is freely permeated by the chemical rays. It is curious that several *yellow* metallic solutions, as the chloride of gold, the chloride of platinum, the permuriate of iron, and the hydrosulphate of lime, act about as powerfully as the chromate of potassa, but this peculiar tint is not always effectual in producing this result, for the yellow oil of turpentine, and the yellow ferrohydrocyanate of potassa, fail to prevent the blackening of the chloride.

These experiments, therefore, decide the question of the separate existence of calorific and chemical rays in solar light; they also enable the philosophic chemist to insulate each ingredient and operate upon it by itself, a matter of the utmost importance in the investigation of the properties of light.

Ibid.

Presence of Iodine in various minerals, and in plants which grow remote from the Sea.

Iodine was first discovered in the ashes of sea weed; afterwards in sponge and in the waters of some mineral springs. This singular substance seemed therefore to be justly ranked among marine productions, when, to the astonishment of chemists, Vauquelin detected it in considerable portion in a specimen of an Argentiferous mineral which had been sent him from Mexico. No one knew, however, from what locality this mineral was derived.

Recollecting these circumstances, M. Arago announced to the Academy, that having fallen in company with some young officers of the corps of engineers which the Mexican government had just sent to Paris to continue their studies, it came into his mind to speak of Vauquelin's discovery, but without any expectation whatever, that these young *militaires* could supply the information which was wanting with respect to the locality of the mineral in question. We may imagine how agreeably he was surprised in receiving from Captain Yniestra a note, of which the following is nearly a literal translation.

"At the time when Vauquelin discovered Iodine in a silver ore, from Mexico, M. del Rio, professor of mineralogy at our School of Mines, proved the presence of the same substance in the pure silver of Albarradon. This is the name of a district contiguous to that of Mazapil in the department of Zacatacas. Temeroso is the name of the mountain of Albarradon, where the silver mines are situated.

"Our noted Bustamente has also found Iodine in the white lead of the mine of Catorce, situated in the department of Guanajuato. In 1834, in company with Mr. Herrera, I made myself a quantitative analysis of this last mentioned mineral. I will give you the results when my trunk arrives.

"I know not whether you have been informed, that in Mexico, Iodine has been discovered in the Sabila and the Romeritos. The Sabila is a plant of the genus maguey *(agaves)* which grows on the plains and on the sides of mountains. The Romeritos is a kind of varilla which vegetates on the floating gardens of the fresh water lakes in the neighbourhood of the Capital, it is much eaten during lent."

Annales de Chemie.

Ibid.

Rapid Congelation of Water by means of Hydric Ether and concentrated Sulpuric Acid, &c. *By* R. HARE. M. D.

In freezing water by the vaporization of Hydric, commonly called Sulphuric Ether, there is much labour in pumping, and the etherial vapour condensing in the pump, disqualifies it for nice experiments until cleansed. Dr. Hare finds that the interposition of sulphuric acid lessens the requisite labour, and protects the pump. By means of a globe or bottle with two tubulure, and a glass funnel with a cock, the acid being in the globe, the water in a retort, and the ether in the funnel, while the two former are exhausted, on allowing the ether to descend upon the water, the congelation of this liquid is instantaneous.

It has been ascertained by the same chemist, that a permanent self-regulating reservoir of chlorine, may be made by means of the apparatus heretofore used by him for nitric oxide, substituting for the materials used in that case, manganese in lumps and concentrated muriatic acid.

In one case, Dr. Hare, doubting the purity of the gas, from some indications, among others the want of the usual degree of colour, in order to test it he exposed leaves of a thin metal called Dutch gold leaf, to a jet of this gas as he had previously done repeatedly, without any ill consequence; to his astonishment, an explosion took place, which burst the apparatus and produced a detonation as loud as if one of the explosive compounds of chlorine and oxygen had been generated. Yet the only agents employed were perexide of manganese and chloro-hydric (muriatic) acid. It was the deficiency of intensity in the colour which led him to test it by means of the leaf metal. The colour of the protoxide is known to be of a deeper yellow than that of chlorine.

Ibid.

ANNALS OF ELECTRICITY &c

PLATE.XI.

Fig. 76.

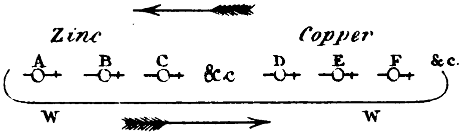


Fig. 77.

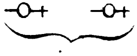


Fig. 78

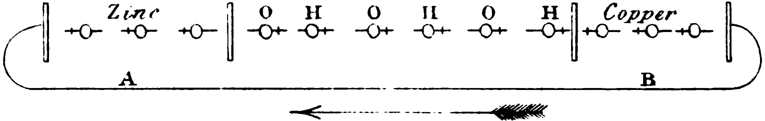


Fig 79

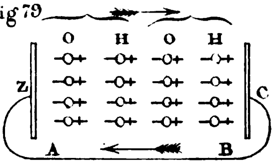


Fig. 81.

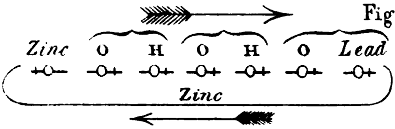
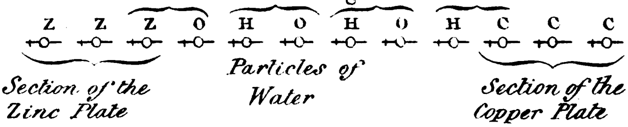


Fig. 80



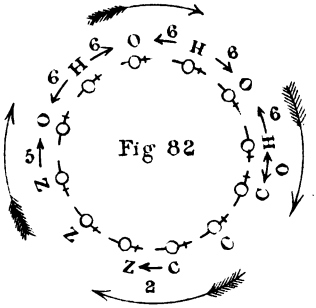


Fig 82

Fig. 86.

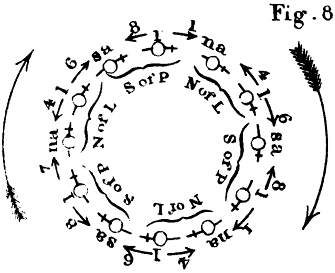
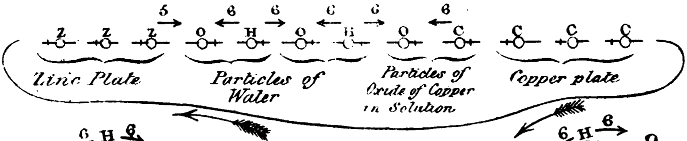


Fig 83.



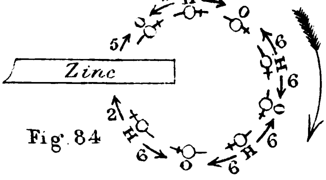


Fig. 84

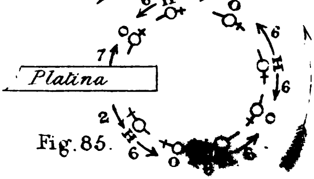


Fig. 85.

W. Annan. lith: 12 Gracechurch St.

PLATE XII.

Fig 87.

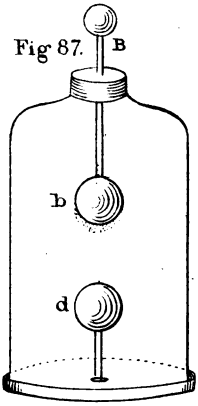


Fig. 88.

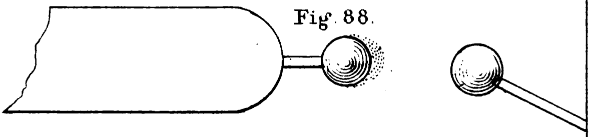


Fig 89

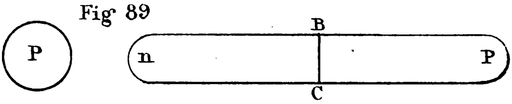


Fig 90

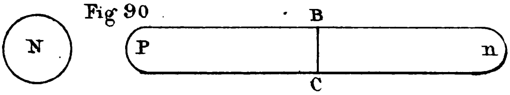


Fig 92

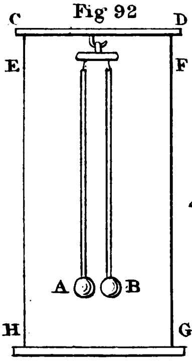


Fig 91.

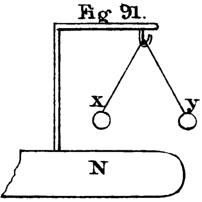


Fig 93

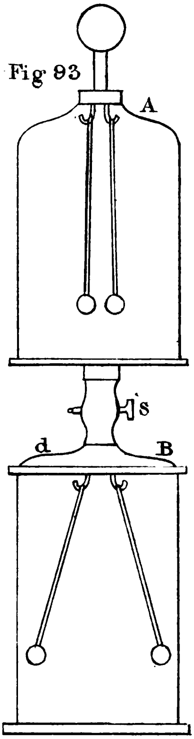


Fig 94

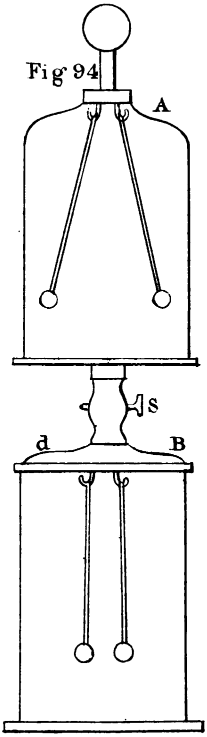


Fig 96.

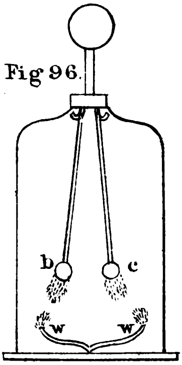
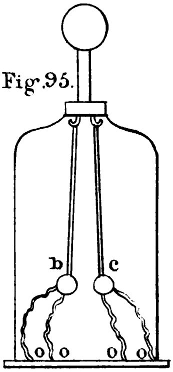


Fig 95.



W. Annan lith. 12 Gracechurch St.

THE ANNALS
OF
ELECTRICITY, MAGNETISM,
AND CHEMISTRY;
AND
Guardian of Experimental Science.

JUNE, 1838.

LXIV. *Experimental and Theoretical Researches in Electricity. First Memoir. By* William Sturgeon, *Lecturer on Experimental Philosophy, at the Honourable East India Company's Military Seminary, Addiscombe.**

SECTION I.

Read December 5th, 1837.

Different opinions of philosophers respecting the nature of electric action—The vibratory Hypothesis examined—Its principles not analogous to those of the hypotheses of Sound and Light—Evidences of the existence of an electric matter.

1. The memoir which I am now about to offer to the notice of the Electrical Society may be considered as the first of a series which it is my intention to bring forward as speedily as circumstances will permit. These memoirs will exhibit a selection and arrangement of facts, which, if I have not deceived myself, can hardly fail to have some weight in the reasonings of those philosophers whose opinions are not yet reconciled to each other respecting the *modus operandi* in the production of certain electrical phenomena.

2. In an enquiry of this kind it often happens that, notwithstanding the apparently trivial circumstance to which it is mainly directed, it becomes essentially necessary not only to notice, but to investigate, certain other points with which it is obviously connected, in order to satisfy the mind respecting the bearing and influence which those points have upon each other; as also, on that which is the principal object of pursuit.

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* From the Transactions of the London Electrical Society.

3. Philosophers, of the present day, have taken such extremely dissimilar views of the nature of electric action that they have imposed upon us the indispensableness of a minute retrospection of almost all the variety of phenomena that have hitherto been developed; the laws of whose exhibition necessarily contribute to the establishment of those impenetrable and unalterable principles upon which the science must ultimately rest; and have demanded a reinvestigation of even the very arcanum of all electric action.

4. A certain class of these philosophers who have undertaken the explanation of electric phenomena, consider it requisite to combine the operations of *two* distinct kinds of electric matter, which they have called the *vitreous* and the *resinous*; independently of which, they imagine, no electric phenomena can possibly exist. A second class have contented themselves with the management of *one* fluid only; whilst a third class, still more economical than the preceding, have undertaken the solution of every electric problem, hitherto discovered, independently of the operation of any electric matter whatever; by supposing that electric phenomena are the effects of certain rotatory, or vibratory, motions of the particles of the common matter composing those bodies on which they are displayed. Each of these hypotheses is supported by men of the highest respectability, and of acknowledged talent. They are become subjects of much important discussion amongst electricians of every country; and, therefore, a rigid and impartial investigation of the principles on which they are founded is the only mode of proceeding which can satisfy the mind as to the intrinsic value of their respective pecuiiarties, and to form a proper estimate of their individual claims to attention. The results of such an enquiry, if properly conducted, can hardly fail to be interesting to the Electrical Society; whilst, in the present instance, it may be regarded as an important preliminary step unavoidably touched upon in the path of research I have ventured to pursue.

5. In an undertaking of this magnitude and importance, under circumstances embracing a balance of authority amongst those who have taken these very different views of electric action, much caution and rigorous circumspection ought necessarily to be observed in every stage of the enquiry. Moreover, the present infantile state of this Society imperatively demands that a copious selection of obvious and unequivocal experimental data be advanced, and that all reasoning therefrom be plain, lucid, and familiar.

6. Respecting the order in which these hypotheses come under consideration, the precedency would have been but of

very little consequence had the probability of truth appeared equally favourable amongst them, or that they had differed from each other in some trifling peculiarity of detail only; but as there is such a wide difference in the very basis of these doctrines, and especially between those which admit of the existence of an electric matter and that which precludes it altogether, it appears essential that the mind becomes perfectly satisfied, as early as possible, respecting the nature of the evidence on which these opposite theoretical views have been founded, and by which they are the most likely to find support; in order that the investigation may be facilitated by disposing of those first which appear to have the least probability in their favour. Moreover, as the existence of an electric matter has, for a long series of years, been acknowledged by almost every philosopher who has paid a sufficient degree of attention to the subject to enable him to form an unbiassed opinion, resting on experience alone: and that the vibratory hypothesis appears more like a novel creation of the imagination than as a doctrine founded on observation and fact, and still remains little more than a confusion of discordant surmises, without, even the least pretensions to systematic organization, or the shadow of either law or rule for the guidance of the electrician, there can appear no impropriety in commencing with a brief enquiry into the extent of interpretation of electrical phenomena, which the latter hypothesis is capable of affording: and afterwards examining that which has so long rested on the supposition of the individuity of an electric matter; and which has obtained a code of laws supposed to be sufficiently explanatory, as far as they proceed, of every fact hitherto developed in certain branches of electricity.

7. If the hypothesis, first for consideration, supposes that electric action depends upon, and emanates from, vibratory movements of the particles of those kinds of matter of which the apparatus employed are usually constructed, such as metal, wood, glass, &c., independently of any other agency, there could be no difficulty in showing its entire fallacy: because it is a fact easily ascertained, that no very energetic electric action, if any at all, can possibly be exhibited by any vibratory movements which those bodies are susceptible of receiving by mechanical means; although those vibrations might be infinitely greater than could possibly be produced by any other mode of procedure. Nor can I think it possible that any electrician, of the present day, whatever may be his theoretical views, or however fond he may appear to be of novel modes of explanation, would be found so far deficient in electrical knowledge as to hazard his fame by an attempt to

charge a Leyden jar by the mere vibration of a mass of copper, glass, wood, or any other solid body whatever.

8. If, again, it can be supposed that the charge of a Leyden jar or any other piece of glass, consists in certain tremulous motions of one, or more, of the materials of its structure, communicated from the prime conductor of a machine; is it possible to imagine that those motions would continue during the weeks, nay, even months, that jars have been kept in an electrized condition after they had ceased to be in connexion with any electric apparatus? There is no evidence of such continuous quiverings of the glass, nor is there a fact known to induce a belief that any tremblings exist in the instrument even for a few moments after it has been taken from the machine; nor indeed any whatever, only such as are occasioned by the unavoidable shaking attending the working of the apparatus.

9. Can any one persuade himself that the electric action exhibited by two morsels of metal, by simple contact only, is occasioned by tremulous motions communicated to the metallic particles by the pieces just touching one another? Can he, moreover, stretch his imagination so far as to satisfy himself that the metals constituting a dry electric column will continue to quake for years after the instrument is first made; and that, notwithstanding all the care that is taken to prevent any motion amongst the materials of the pile, the electric phenomena exhibited by it are the mere effects of a tendency to motion which these materials naturally possess, and which keeps them trembling in spite of all the efforts of the workman, and contrary to the laws of inert matter?

10. I am well aware that the favourites of this hypothesis build much of their reasoning upon supposed analogies: and particularly from the doctrines of sound and light; wherein it is considered that all the variety of phenomena constituting accoustics and optics are the effects of undulatory motions of matter, which, in itself, is neither sound nor light. But it must be borne in mind, that sound is not produced without a first cause. The sonorous body must first be agitated before it can be productive of sound; and the surrounding medium must be agitated to the ear in order to convey the proper impressions to the tympanum, otherwise the sensation of sound cannot exist.

11. Light, also, requires a first cause to shake the medium which is supposed to be productive of it. Moreover, the medium itself is supposed to be *peculiar*, and alone appropriate to the exhibition of the phenomena of light. Hence it is obvious, that unless the electro-undulatory theorists admit of

the tremulous motions of some peculiar species of matter, they find but little support from analogy, as far as regards the doctrines of sound and light. And an unequivocal concession of this point, would dispossess the hypothesis of the most material peculiarity discoverable in its structure, by admitting of the existence of an electric matter; and whatever appellation might be conferred on it, the very idea of its existence, and of the indispensableness of its presence in the production of phenomena, would be sufficient to assimilate this hypothesis with those it was intended to subvert. Or if there could be any real difference, it would consist in the substitution of *vibrations* for *transmissions*.

12. As it is not my intention to enter into a historical account of the respective hypotheses which have been contrived for the explanation of electrical phenomena, it will be unnecessary to bring forward the names of individuals who have attempted to support that to which they have been most decidedly attached. Moreover, the hypotheses of Du Fay and Franklin are so well known to electricians that it would be quite unnecessary, at the present day, to go through a detail of the principles on which they are respectively founded. And, although we are very differently circumstanced with respect to the vibratory hypothesis, there being no work in which its principles are intelligibly developed, and consequently no source from which much useful information can be collected, it would be idle to discuss phantom theories which no one would claim; and which, in consequence of the vague indeterminate manner in which they have been ventured upon the credulity of philosophers, it would be difficult to trace to any origin by which they could not, without any refined sophistical dexterity, be very easily evaded. Indeed, I am not aware that any philosopher has attempted to lay down a *plan* for an electrical hypothesis in which a peculiar species of matter is entirely dispensed with: although there are some of them, of considerable eminence, who have ventured an *opinion* that the existence of such matter is not essential to the production of electrical phenomena: assuming that some *undefined* motion amongst the integrant particles of those bodies usually employed in the experiments are alone sufficient for the explanation. It would be exceedingly difficult, if not totally impossible, to form a rational idea of any mode by which such motions of the particles of solid matter could possibly exist. Such an idea appears insusceptible of demonstration, and bears not even the slightest token of probability. But admitting even the existence of these supposed motions, under certain circumstances, they would have to be regarded as *effects* rather than *causes:*

which, like all other effects, would be referrible to some preexistent force, however mysteriously that force might operate, or however its operations might be concealed from mental perception. The friction suffered by the revolving glass, or the stationary cushion of a machine, might possibly be construed into a cause sufficient to produce the supposed atomical motions: but its efficacy in this respect, would require an elaborate strength of the imagination to continue it to electrized jars long detached from the prime conductor, and far removed from all other electric apparatus.

13. Any attempt to trace the polarity of the dry pile, or the electric condition of a still atmosphere, or any other electro-statical phenomena to the supposed restlessness of the particles of the metals, or of the air, would have to be ventured under the most unfavourable circumstances that could possibly have accompanied it: viz. in the absence of both fact and analogy. Every vertical column of a dry cloudless atmosphere, whatever may be its dimensions, is constantly electro-polar in one and the same direction, having its positive pole upwards.* But it would be difficult to reconcile the mind to a belief that this circumstance is solely owing to the intestine motions amongst the particles of the air, and independently of any other agent. Again, the two sides of a single piece of thin metal, the one bright and the other dull;† or both bright but of different degrees of polish, are as decidedly electro-polar as the two surfaces of an electrized jar; or of the two extremities of the most extensive voltaic arrangement: and it appears to be of little consequence how thin the metallic piece may be

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* I have made more than five hundred experiments with kites for exploring the electricity of the atmosphere; and, in every case, when clouds do not interfere, I have found the upper strata positively electrical, with reference to those which are below. I have had three kites, and sometimes five, at different altitudes at the same time: and have transmitted sparks from one to another from the top to the bottom of the series: in every case I found the uppermost of a pair, to be the positively electrized stratum of the atmosphere: so that if the strata in which the kites were immersed were at altitudes corresponding to the series 1, 2, 3, 4, 5, their relative electric states would be very conveniently represented by those numbers. The experiments were made at different seasons of the year, and in every part of day and night. When clouds interfere, the distribution of electricity natural to an unmolested atmosphere is often disturbed; and other phenomena occur which will be more particularly noticed in a future memoir.

† See my "Recent Experimental Researches in Electro-magnetism and Galvanism, part 1, 1830.

for this development of electro-polarity: but it would be ridiculous in the extreme to refer this circumstance to the imaginary quiverings, or rotations of the metallic particles constituting the piece. I have now in my possession, some hundreds of pieces of thin zinc, each of which has had its two surfaces in opposite electrical conditions for more than ten years; but I have never attributed their electro-polarity to any quiverings or other motions of the metallic particles; nor can I conceive that such an idea is possible to be formed either from any direct fact, or even from the most remote analogy. Light certainly pervades solid transparent bodies: but it is not considered essentially necessary that the solid particles of those bodies should become agitated for its transmission: or if such intestine motions of the transparent body even were required to accommodate the theory to the fact, still they would have to be acknowledged as effects and not causes: and even the waves of light themselves, which produced those effects, would have to be traced to a still more remote cause, which itself might be discovered to be an effect of the primitive disturbing force.

14. The ticking of a watch, or the scratching of a pin at the remote end of a long piece of timber, is more distinctly heard by an ear, placed at the other end, than if no such substance had intervened; but whether the explanation of this fact were to rest on the supposition of the impressions being communicated to the ear through the medium of the air in the capillaries of the wood, or on the incomprehensible vibrations of the solid mass, the immediate causes of those movements would still have to be referred to those of the watch, or the pin: and those again to the respective forces which put these articles into motion. Hence, in these cases, as in all others, I have had occasion to notice, the phenomena of light and sound are to be attributed to extrinsic causes, there being not the slightest evidence in favour of the supposition that either class of phenomena is a mere consequence of an innate corpuscular motion of those media which immediately transmit the appropriate impressions to the respective organs of sight and hearing: and consequently no analogies discoverable, from which an idea could be formed, of any innated atomic motions of the metal being the cause of electric-polarity in the pieces of zinc already mentioned (13). Having mentioned the motions of a watch as being referrible to another cause which is obviously traceable to the main spring; and as this part of the machine appears to exert a force of its own accord, it may possibly be imagined that the hypothesis I have been discussing would find a favourable analogy in that circumstance. But it must

be borne in mind, that unless the spring had been first moved by some other force it would never have exerted any power over the other works of the watch; for whilst in its original form, and unmolested, it is perfectly inert. Hence, the assumptive analogy again fails; and I believe no analogy is to be found within the precincts of physical science.

15. Whether electric phenomena be regarded as a mere variety of an extensive class, including those of light, heat, and magnetism, or as a distinct kind traceable to an individual source, the probability of an electric agent would be very great, and much favoured by analogy: but there are other sources of information of much greater importance, from which inferences may be drawn and satisfactory conclusions formed on this part of the electrical hypothesis. The indications of the existence of an electric matter are so various and extensive, that one would almost wonder how any idea to the contrary could possibly have been formed. The mechanical and calorific phenomena of electricity are those which are most usually recognised as the productions of an electric agent; although, I believe, there are none hitherto developed, that might not as easily be traced to the same cause. The displacement of granular substances; the perforation of compact bodies; and the fracture of those which are brittle, such as glass; and the violent blows given to animals by electric discharges, are all indicative of the action of some agent of considerable mechanical force. Moreover, these effects can be augmented almost to any extent, or they may be abated so as to be scarcely discernible: and this under circumstances wherein it would be said, in electrical language, that the same quantity of fluid were in motion in every case. If, for instance, the electric force excited by fifty turns of a machine, were to be collected in a high state of intensity on the surface of a jar, and afterwards discharged in the usual manner, through a metallic circuit, in an opening of which were placed a man, a pile of card paper, or a granulous substance, such as loose sand, or gunpowder; a violent blow would be given to the man, the pile of paper would be perforated or even torn to pieces, or the gunpowder would be blown away from the spot without ignition; each fact indicating a mechanical force of considerable intensity: and even of the transmission or passage of some material agent. But if, instead of the whole circuit, (unoccupied by the man, paper, or gunpowder), consisting of metal, a portion of it, amounting to five or six inches, were of a thin strip of water, or a moistened thread of cotton, silk, or any such material, no such mechanical effects would be produced. The man would experience no shock, the paper

would not be torn, nor would the gunpowder be scattered as before; but it would now be set on fire.

16. It would be exceedingly difficult to reconcile these phenomena to any self vibratory motions which could be imagined to exist in the materials of the circuit, or to any motions of this kind pre-existing in the jar and transferred to them. But, by admitting the existence of an active electrical agent, distinct from the solid and liquid parts of the apparatus, we are able to find an easy and natural solution to the problems which these varied phenomena present.

17. By assuming that the fifty turns of the machine, forced a certain measure of the electric matter on to the surface of the glass, and that through the metallic circuit this matter was enabled to move with great celerity; we have then all the data necessary to satisfy the conditions of an electro-momentum of great energy, which is amply manifested by the effects it produces. But, on the other hand, when the same quantity of the electric matter is transmitted through the moistened thread, or any other inferior conductor capable of retarding its velocity, the momentum would obviously be lessened upon the strict principles of matter in motion: and the mechanical effects upon bodies placed in the circuit, would be proportionally abated; which is conformable to the results of the experiments.

18. If it can be imagined that with the metallic circuit the vibrations were more powerful than when a part of it consisted of inferior conductors, and that the mechanical action was increased accordingly, we should be under the necessity of allowing, that the less degree of vibratory motion is essential to the ignition of the gunpowder. But how should we be enabled to reconcile this latter conclusion to other facts in which calorific effects of electric discharges are so eminently displayed? A piece of steel wire is ignited by an electric discharge, under no other circumstances than when the entire circuit is metallic, or at least of very good conducting materials: and the more completely is the conducting powers of every other part of the circuit maintained, the more probable it is that the thin steel wire will be ignited. If the steel wire be short, it may be fused by a discharge from a moderately sized jar: but if it be long, a similar discharge would not make it visibly red hot: and by having a moistened thread in the circuit, the thin steel wire would develop no conspicuous signs of even an elevation of temperature: although, as has already been shown, the latter conditions are those alone under which the gunpowder will ignite. From these facts we are led to understand that different inflammable substances require

different modes of treatment to accomplish their ignition by electric agency; whilst in a mechanical point of view, the character of the substances operated on requires no peculiarity of circuit for the production of similar effects: for invariably the mechanical action is greatest with the best conducting circuit, and abates gradually as the circuit becomes less and less perfect in its conducting character.

19. At the time I was making my experiments on the ignition of gunpowder by electric discharges, I was well aware of the necessity of varying them in every possible manner that I could think of, and the result of one of these experiments appears to have led to some doubt of the correctness of the theory which I then advanced to account for the cause of the action; the principal part of which may be embraced in a few words as follows. When the discharge is made through good conductors; as copper wire, for instance, the electric matter passes through the gunpowder with so great a velocity that it has not *time* to ignite it; but when that matter is retarded in its progress by having to traverse inferior conductors the *time* occupied to pass through any transverse section of the circuit is sufficiently great to accomplish the ignition.* This explanation has been objected to because it is a fact that in whatever part of the circuit the wet string formed a part of it, the gunpowder invariably ignited; and because the ignition was accomplished when the wet string was placed on what is usually called the *negative* side of the gunpowder, it has been thought that the string could have no part in retarding the motion of the electric matter whilst traversing the gunpowder. This objection, however, may be very easily removed by assuming the electric matter as a highly elastic fluid; and contemplating the phenomenon in question upon the principles of elastic fluids generally. If, for instance, a reservoir of condensed air were to be discharged through a tube sufficiently wide to offer little resistance to its motion it would rush through the tube with considerable velocity, driving before it, of course, the air of the common density with which the tube was previously filled; or, in other words, the same quantity of air as that which was liberated from the reservoir would occupy but very little time in passing out at the farthest end of the tube. But if a similar reservoir of air were to be discharged through the same tube, now terminating with another of narrow bore, the time occupied for the escape of

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* See my papers on the ignition of gunpowder by electric discharges. London Phil. Mag. vol. lxvii; and vol. i, of the United Series of the Phil. Mag. and Annals of Philosophy.

the air would be much greater than in the former instance; and precisely the same period of time would be occupied if the small tube were in any other part of the circuit, provided the whole of the air had to traverse it. If now this reasoning be transferred from the fluid air to the electric fluid, it will lead to similar conclusions, and show that if the velocity of the fluid be checked in any one part of the circuit it will also be checked in every other part of it.

20. That the time occupied for a discharge through the two kinds of circuits is different, being much greater in one case than in the other, is so exceedingly obvious, that no one acquainted with the experiments would attempt to deny the fact. Notwithstanding, however, it may possibly be necessary, in this place, to mention some of the appearances and effects by which it is most easily attested and understood. When the circuit is completely metallic, with the exception of a small opening in any one part for the purpose of examining the electric light, the jar is completely discharged by the shortest possible contact of the discharging rod; or, in other words, by a mere momentary closing of the remaining part of the circuit; and the electrical light at the opening, is seen but for a moment, is exceedingly brilliant, and attended with considerable noise. But when the wet thread forms a part of the circuit, the jar is not so suddenly discharged, the light is seen for a considerable time at the opening, and its former brilliancy has entirely disappeared; being now reduced to a mere redness, and attended with scarcely any noise whatever. If, whilst the circuit was metallic, a finger were placed in the opening during the discharge, the finger would receive a severe blow, and for a moment be highly illuminated within; but by employing the aqueous circuit no illumination would take place, neither would anything like a blow be experienced. The mechanical action of an electric discharge is so far abated by having a portion of the circuit of moistened thread, that the most delicate child might be placed in its way without experiencing the least inconvenience; indeed scarcely any sensation is discernible; yet in the very same circuit gunpowder might be ignited. I have frequently placed young persons in such a circuit during the discharge from six square feet of coated surface on each side the glass, charged to a high intensity. These persons never experienced any disagreeable sensations from the electric action, although eight pieces of miniature ordnance placed in other parts of the circuit have been discharged by the same electric influence. But if the circuit were to be completely metallic a similar electric discharge would produce such a violent blow that the

stoutest man would not like to experience the sensation a second time.

21. In all these phenomena we have unequivocal signs of the existence of an electric matter, whose mechanical effects are as decidedly modified by varying its velocity as are those of any other species of matter whatever; and whose light also, is more and more intense as its density increases, but which becomes faint as it is attenuated, and viewed in a less compact body.

22. I am well aware that some philosophers are of opinion that the brilliant light which is developed by the electric spark, may possibly occur from a sudden displacement and subsequent collapsion of the atmospheric air, the caloric of which becoming sensible and luminous by compression; founding their reason, principally, on the fact that the air, when discharged from an air gun, produces light. Now, admitting this to be the case, no one could suppose that these effects are produced independently of an adequate force; and a physical force invariably implies the existence of active matter. But we have no idea—certainly no proof—of a piece of glass or a piece of metal being thus active; hence we are constrained to admit of the existence of some other agent in the production of these phenomena. Moreover, it is well known that although the electric light is not so brilliant in attenuated air as in that which is more dense, its existence is still manifested in a very striking and beautiful manner. Our imitations of the aurora-borealis are highly demonstrative of luminous electric matter. Besides, if there can be any discernible analogy in the light given by an air gun, and that of an electric spark, it must certainly be exceedingly remote; and the mind in which it is formed susceptible of more delicate impressions than that generally implanted in man.

23. The luminous phenomena calculated to attest the existence of an electric matter are various and extensive. The pencil, the star, the cascade, the falling star, and the electric meteor, exhibited over the surface of moist conductors, are amongst those which appear insusceptible of explanation independently of an electric matter: and the splendid bow between charcoal points attached to the poles of a voltaic battery, is untraceable to any other cause.* No one, at the

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* There is another beautiful phenomenon of electrical light, which is not so familiar to many persons, as those mentioned in the text. It is the *luminous electro-sphere* exhibited on a positively charged conductor. The experiment is made in the following manner:—let fig. 87, Plate XII., represent a glass receiver, furnished with a collar of leathers, and a sliding rod passing through them.

present day, doubts the identity of electricity and lightning; and can there be a mind sufficiently impervious to external impressions as to doubt of lightning consisting of, or emanating from, a peculiar and active agent? And what physical agents are there which admits not of materiality, either directly or indirectly? And what known agent but the purely electric is

The rod has a brass ball at each extremity. This receiver is to be placed on the plate of an air pump, from the centre of which rises a stout brass wire stem, surmounted with a ball. The ball *b*, is to be adjusted at about five inches distant from the ball c, and the air in the receiver to be attenuated by the action of the pump. If now the ball B be brought close to the prime conductor, whilst the machine is in good action, a beautiful luminous electro-sphere will be seen covering the lower side of the ball *b*; but no light on *c*. If a moveable pump plate be employed, and the whole removed from the pump, the instrument may be held in the hand by the brass cap on the top, and the plate brought close to the prime conductor. The lower ball c, will then have its upper surface adorned with the luminous electro-sphere; and no light will be seen on *b*. This experiment was first described by Father Beccaria, in his Treatise of Electricity, published at Turin in 1753.

I have frequently repeated this beautiful experiment, and have found that the luminous electro-sphere can be exhibited without the aid of an air pump; even in the open air. Considering, as others have done, that the pressure of the air is one of the principal causes of the electric matter being kept either within, or close to, the surface of charged conductors, it appeared likely that a partial removal of that pressure was the reason of the appearance of that matter in Beccaria's experiment; and if so, why not its appearance in the open air with a stronger electric charge in the conductor? To ascertain how far this reasoning would be sanctioned by experiment, I put my ten inch cylindrical machine in excellent order, and made the room completely dark.

The machine was kept in vigorous action by my assistant, but it was not till after some considerable time had elapsed, that I saw the electro-sphere on the ball of the prime conductor. This occurred after a great number of fine sparks had been taken from the ball; a process which I have since found conduces to the exhibition of the phenomenon; though by no means essential to its appearance.

Fig. 88, will serve to represent the remote extremity of the prime conductor, and its ball, with the luminous electro-sphere. When the machine is in good action, I never fail to see this light on the ball after an exhibition of the aurora borealis experiment, when the receiver is removed from the ball of the conductor. And, on some occasions, when no ball has been attached, I have observed a similar light partly enveloping the most remote convex surface of the prime conductor; though more frequently, this light caps a few prominences only, which stand amongst the indentations occasioned by accidental blows which that extremity of the conductor has received.

endowed with the activity and energies of lightning? Or capable of producing those tremendous effects universally acknowledged to be attributable to this power alone? I am well aware that some philosophers are more prone to *doubt* than admit any theoretical point which they themselves have had no share in establishing: and the philosophical reputation of some men rests, principally, on a steril system of *doubting*, which they gravely and inflexibly pursue. But even the most sceptic are sometimes led *indirectly* to an acknowledgment of theoretical explanations, which their proneness to doubting would not allow them directly to admit. Every philosopher who has contemplated the phenomenon traces the *immediate* cause of thunder to a sudden collapsion of displaced air. The acknowledgment of this undisputed fact admits, without further evidence, of the existence of an electric matter, which first displaced the atmospheric air; and however reluctantly we might be inclined to concede to the fact, the long line of space from which the sound originally proceeds, constrains us to believe that the vacuum was not limited to a point, or to a small sphere of space; but that it was elongated, and produced suddenly and without interruption at very different distances from the observer: implying thereby that the matter which produced it was in very rapid motion. Nay, what observer has not seen lightning traversing a long track of atmosphere?

24. The mechanical phenomena producible by electricity are so exceedingly numerous and obviously demonstrative of the materiality of their origin, that, independently of any other, they alone afford abundant evidence of the fact. If a hard steel bar be placed vertically, or even horizontally, with its axis in the magnetic meridian, it becomes magnetic by submitting it to a violent electric discharge: but if the force of the electric discharge be so far abated, by transferring it through inferior conductors, as not to produce a sufficient degree of agitation of the steel, no such magnetic effect takes place. These effects are precisely those which would be produced by the blows of a hammer, or by any other mechanical power. Smart blows, sufficient to agitate the steel, gives an opportunity for the earth's magnetism to polarize the bar; but when the blows are feeble, no such magnetization is produced.

25. If a discharge be directed through a piece of wood, the latter will be cleft, or split into shivers. A piece of soft clay becomes disturbed, and hollow in the middle, or shattered to pieces, by a similar process. If the point of a bent wire be placed against the inner surface of a glass phial filled with oil, and sparks be taken at the other end of the wire, the glass soon becomes perforated; and many perforations may be made

in a short time by moving the point to different parts of the glass, and holding the finger opposite to it on the outside. In all these instances, and in many more that might be adduced, we have direct evidence of the operations of peculiarly active matter; whose powers are still further manifested by its grinding to an impalpable powder the side of a jar, or other piece of glass, through which it has forced its way.

26. Besides the tangible, ocular, and auditory manifestations of the operations of an active subtile species of matter which appears essentially existent in the display of electrical phenomena; the olfactory and palatic organs also, bear testimony of its peculiar impressions. Every electrician is perfectly familiar with the remarkable odour developed by a machine in good action; and the peculiar tartness produced in the mouth by the application of two morsels of connected metal to the tongue, or the polar wires of a voltaic battery to the opposite sides of the face, is also well known to most persons accustomed to the use of these apparatus.

27. The former effect is producible in a variety of ways, by some of which it may be retained for a considerable time after the machine has ceased to be in motion. The room in which a machine has been working for some time, will evince electrical excitement, to any one habituated to the specific odour, for even an hour or more after the process has ended. And the aurora borealis experiment never fails to leave a strong electric odour in the receiver for a long time after it is taken from the pump plate. This latter fact is the more remarkable and important because of the odour being produced in the vessel when nearly exhausted of atmospheric air; and appears to militate against the idea of its production from secondary causes; especially from those chemical changes which might have been supposed to have taken place in the atmospheric air; unless it can be proved that such changes are more easily accomplished when that air is much attenuated from the natural standard of density at the earth's surface. But even admitting that the smell and taste so eminently distinguishable by electric action, are secondary effects, their testimony of the entity of a primitive electric agent would be no less manifest; because the chemical changes themselves would be referrible to that agent; since neither the elements of the air nor of the saliva evinced the least tendency to such change either prior or subsequent to the electric process. Hence we discover that every organ of sense is more or less, directly or indirectly, susceptible of impressions from electrical phenomena, which transmit to the mind those special kinds of intelligence for which they are respectively and appropri-

ately adapted: and it is by the intelligence which these impressions communicate, and by these alone, that our ideas are to be formed, our reasoning regulated, and our decisions ultimately arrived at respecting the entity or non-entity of an electric agent. Moreover, from the impressive evidence derivable from this source of intelligence, of the existence of an electric agent, and the total absence of facts, or even strict analogies from which inferences could be drawn to the contrary, we are constrained to acknowledge the entity of this agent, and to abandon the idea of accommodating electric phenomena to the indiscriminate, and hitherto undescribed motions of those bodies on which they are usually exhibited.

28. To enumerate all the facts which manifest the existence of a peculiar matter from which electrical phenomena emanate, would be an unnecessary labour for the present purpose. They would require a volume for their description, and much time for their arrangement and explanation. I have brought forward those only which appear most conspicuous to common observation, and perhaps, best known to the greater part of those persons in whose hands this memoir is likely to be placed; and, at the same time, sufficiently obvious to be understood, even by those with only a moderate degree of electrical knowledge. Moreover, as I am perfectly familiar with every fact that I have adduced, and contemplated them with much care, I labour under no apprehensions of being suspected of entire ignorance of my subject; and have no hesitation whatever, in submitting this investigation to the candid scrutiny of the ablest electrician. In every part of it there has appeared to me, full and unequivocal evidence of the existence of an electric matter; and which I have been led to believe is perfectly distinct from all others, even the *magnetic* and *calorific* not excepted.

SECTION II.

Read February 3, 1838.

" *Historical Evidence in favour of the inferences drawn in the preceding section.—Electric fluid.—Du Fay's opinion of two electric fluids.—Watson and Franklin's idea of one electric fluid, sui generis.—The author's theoretical views.— Electrical attraction.—Electrical repulsion, and its analogies in physical science.—Electro-polarization by locality.—Various explanations of this phenomenon.—Lord Stanhope's electroscopic experiments examined.*

29. The first part of this memoir has already been read before the Electrical Society, and is solely devoted to an enquiry into one of the fundamental elements of the theory of electricity: for, being the commencement of a series of memoirs which I have undertaken to bring forward, for the purpose of conveying a comprehensive view of the various classes of electrical phenomena, and of the laws which appear to give them existence, it was deemed necessary, in the first place, to become perfectly satisfied respecting one grand theoretical particular; on which much subordinate matter seems mainly to rest; but concerning which, a very great difference of opinion has latterly been entertained by philosophers of the first degree of eminence in this branch of physics.

30. The grand controversial point, or theoretical question, will be the most intelligibly enunciated in the following manner.—Are electrical phenomena traceable to the operations of a material elementary agent, peculiar in its character, and distinct from every other species of matter? Or, can those phenomena be more easily accounted for independently of the operation of such an agent?

31. It is somewhat singular that, notwithstanding all the evidence of the best electricians that the world ever produced in favour of the entity of an electric matter, an opposite opinion should *now* be started without the slightest foundation for its support, or even one single fact in its favour. For my own part, I have so long been convinced of its existence, and founded my reasonings so completely on its operations, that nothing but a profound respect for the candour and intelligence of some of those who have speculated on the novel hypothesis, and expressed their proneness to dispense with the electric matter; and a particular desire to place before this

infant society the principal facts which bear on this point, in a compact and undisguised form, that I could have been induced to devote the time necessary for their reinvestigation, arrangement, and explanation, to this topic. Under these considerations, however, I have undertaken the re-perusal of many authors, and have read some others to which I had, before, not paid sufficient attention to form an opinion of their sentiments, and the value of their personal labours in this department of science. And I have also been induced to repeat several old experiments, and institute some new ones in order to satisfy myself in certain particulars, and make myself perfectly acquainted with every fact on which my reasonings have been founded. The first section of this memoir is principally devoted to these investigations: and the conclusions I have there arrived at, are the natural inferences derivable from the various circumstances connected with the phenomena already explained. I have also extended my enquiries to other electrical phenomena, both mechanical, physiological, magnetic, thermometric, and chemical: and, from a rigid examination of them, and a comparison of the various ways by which some of them have been attempted to be explained, I can discover no hypothesis so free from ambiguity; none so truly specific; none so simple, distinct, and comprehensive: in short, none so apparently rational and conclusive as that which admits of the agency of a purely electric matter.

32. The electric matter was recognised in a very early period of the progress of the science by some of the most active and discerning philosophers of that day. Benjamin Wilson, than whom no one was more conversant with all the then known phenomena of electricity, did not hesitate to attribute their exhibition to the operations of an electric matter; which he was disposed to assimilate to the ether of Sir Isaac Newton; although he gives it the peculiar appellation *electric matter.** Stephen Grey, who made many capital discoveries in electricity, calls this matter the "electric fire,"† which is an appellation given to it by Father Beccaria;‡ and was also employed by Dr. Watson, who says, "that the electrical fire is truly flame, and that extremely subtile."§ Many other philo-

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* Wilson's Treatise on Electricity, 1750.

† Phil. Trans. original No. 436, p. 16; or Hutton's Abridgement, Vol. VIII., p. 5.

‡ Beccaria's Treatise upon Artificial Electricity, Translation 1776.

§ Phil. Trans. Original No. 471, p. 481. Hutton's Abridgement, Vol. IX. p. 158.

sophers of considerable eminence have given to the electric agent the name of electric fire: a term frequently used even at the present day; although "electric fluid" has, in a great measure superseded it. Viscount Mahon, afterwards Lord Stanhope, called it the "electric fluid;"* and Priestley also uses the same term for the electric agent.† Euler appears not so partial to an "electric fluid;" although he frequently employs the terms *positive* and *negative* electricity; and undertakes to explain the whole phenomena by the operations of an highly elastic *ether;* with which the pores of all bodies are continually charged. This ether being susceptible of compression and dilatation would assume different degrees of density accordingly as the pores of bodies were closed or expanded; which was the cause of those bodies becoming *positively* or *negatively* electrical respectively. Euler admits of the transmission of this *ether* from one body to another, and explains the spark, shock, lightning, and thunder in precisely the same manner as other philosophers have done with an "electric fluid," differing from them only in the name of the electric agent.‡ Cavallo was exceedingly cautious in adopting any hypothesis for the explanation of electrical phenomena; yet with all his diffidence, he very frankly confesses that the supposition of a purely electric fluid is certainly the most probable.§ And in another place when speaking of the *residence* of this agent, Cavallo says, "That the electric fluid, proper to a body when in its natural state, is equally diffused throughout all its substance, *I think no one will deny;*"‖ which expresses this philosopher's conviction not only of the existence of an electric matter, but also of the mode of its distribution. The experiments of Du Fay led that philosopher to the belief of the existence of *two* electric fluids; independently of which he could not account for the phenomena which they presented to his notice. The hypothesis of Du Fay has been very much esteemed by the continental philosophers, and by some of them, is still held in considerable repute.

33. In an early part of the year 1747, Dr. Watson made known his ideas of the operation of *one* electric fluid *sui ge-*

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* Principles of Electricity, by Charles Viscount Mahon, 1779.

† Priestley's History of Electricity.

‡ Euler's Letters on different subjects in Natural Philosophy, addressed to a German Princess. Brewster's Translation, Vol. II.

§ Cavallo's complete Treatise on Electricity, Second Edition, p.p. 105, 114.

‖ Ibid. p. 126.

neris;[*] and about the same time in America, Dr. Franklin was digesting his well known theory, which also rests upon the supposition of one electric fluid.[†]

34. The theory of Franklin, as far as it extends, appears to require but very little modification to become applicable to every fact that has been developed in this branch of physics prior to the discovery of electro-magnetism: and, perhaps, it is in this department of electricity alone, where the Franklinean doctrine will be found materially deficient. And even here, notwithstanding its inadequacy to account for this class of phenomena, it does not appear to be materially defective in itself, or physically incorrect: for the principles of that doctrine are as decidedly and as conspicuously in operation in this department of electricity as in any other; and by annexing the principles of electro-magnetism and magnetic electricity to those which Franklin had embraced in his theory, it is probable that we should be in possession of a code of laws, to the operation of which, every known electrical phenomenon may be traced.

35. The same mathematical formulæ are as easily deducible from the vitreous and resinous forces of Du Fay as from the positive and negative forces of Franklin; and, consequently, as applicable to the doctrine of two electric fluids as to that of one electric fluid. But it cannot be said that because mathematical processes are rigidly correct, that such circumstance confers on them the attribute of infallibility in testing the correctness or incorrectness of our notions respecting the physical agency which actuates in the production of natural scientific events. In the instance before us we have a pretty fair specimen of the *flexibility* of mathematics, which are obviously as applicable to false as to true data: for it would be an absurdity to suppose that both these theories are physically correct. The fault, however, is not in the mathematics, but in the data on which the reasoning is founded. The data once given, a moderate degree of skill would enable the mathematician to proceed in his investigation and arrive at results perfectly agreeable to the data. But should the latter be incorrect, the exactness of the investigation could have no tendency whatever to establish a true theory; for the foundation resting on no physical truth, the superstructure itself, in whatever manner it might be adorned with mathematical symbols, would be too frail to withstand those stern facts

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[*] Priestley's History of Electricity, p. 389.

[†] Ibid.

which the inflexible laws of nature develop to rigorous experimental research.

36. Mathematical investigations, however, when directed to physical operations, and based on incontrovertible facts, give a sterling value to science, and establish imperishable sources of conduciveness to the various wants of civilized life.

37. Mechanics, hydrostatics, and pneumatics, are amongst those experimental sciences whose phenomena are calculable with mathematical precision; and which, in consequence, have long become practically and extensively useful. The phenomena of those sciences requiring no dexterous hand for their exhibition, nor presenting any equivocation or capriciousness in their display, were easily reconciled to that degree of certainty and exactness which alone render experimental results susceptible of utile mathematical rule.

38. In electricity, however, the phenomena are of a very different character, presenting difficulties not to be met with in any other branch of physical research. The number of electrical phenomena is so exceedingly great as to surpass the collective sum of all others that science has revealed to man. They are exhibited under such a variety of circumstances both on masses and on the most minute portions of matter, are brought into play by such a diversity of both natural and artificial means, and viewed by experimentalists under such a dissimilarity of aspects, that it is scarcely to be wondered at that the multitude of facts which have been developed in this branch of physics, remain to this day little better than a stupendous heap of inorganized materials; intrinsically rich, but whose real value can never be justly appreciated until they have obtained a natural and obvious classification; and are reduced to some certain inflexible laws, by which they may assume not the habiliments only, but the real character of genuine systematic science.

39. From this view of the present condition of electricity, it is obvious that much still remains to be done before this branch of physics becomes sufficiently matured so as to be established within the precincts of exact science; and, perhaps, it is only from the repeated efforts of close and exact observers, who are well accustomed to the practical part of electricity, that much progress is to be expected in systemizing the phenomena to uniform scientific order; and perhaps, also, to attempt anything further than a mere step in the advancement of electricity, would be more than could reasonably be expected to be accomplished, by any individual in the present disorderly condition of the materials which are placed before him; all of which demand attention, and require the strict scrutiny of

the most acute and profound electrician. I do not, therefore, in this investigation, entertain too sanguine a hope of arriving at satisfactory conclusions on every point which I may have to discuss; though I hope to be enabled to succeed in explaining some particulars which still remain subjects of controversy and dispute. Moreover, as in this series of memoirs I shall have to notice some tributary investigations in which interesting facts have been elicited, whose explanation seems to rest on those very points about which philosophers are still at issue, it becomes essential that I make known, as early as possible, those principles upon which my theoretical reasonings are intended mainly to rest.

40. The theoretical views which I entertain of common electricity are, perhaps, not very different to those embraced in the Franklinean doctrine. I attribute electric phenomena to the agency of a peculiar species of matter, or electric fluid, whose particles mutually repel each other, but which are attractive of all other kinds of matter. By virtue of the innate repulsiveness of its particles, the electric fluid becomes highly elastic; and is compressible and dilatable by the application, or removal, of external forces. The dilatable propensity of the electric fluid gives it a tendency to spring outwards with an equable force in every direction as from a centre; which force is proportional to the force of compression, or to the density of the fluid. The electric fluid is transferable from one body to another; and, like all gaseous fluids, flows with the greatest facility in the line of least resistance.

41. The atmosphere, as far as it has been explored, is continually charged with electric fluid; and in a greater degree as we ascend from the earth's surface (13 and note). From this condition of the atmosphere it is obvious that all bodies on the earth's surface are subjected to an electric pressure, as decidedly as a solid ball of matter would be subjected to an elastic pressure if suspended in an atmosphere of gas. By this electric pressure the fluid forces itself into the pores of all bodies in proportion as they offer facilities for its admission: and as different bodies offer different degrees of facility, they necessarily become charged with it to different degrees of extent. The attractive quality also differing amongst those bodies will also be another means of their being differently electrized under the ordinary pressure.

42. Moreover, as bodies generally, in their natural condition, are compounds and not elementary, it follows that the particles of those bodies which are of a different elementary character are naturally in a different electric condition. Hence, heterogeneous bodies, however compact they may

appear to common observation, are not uniformly electrized; every particle of one of the constituents being in a different electric state to every other constituent element in the compound.

43. The natural electrization of bodies is still further diversified by their mechanical structure and external polish; for a difference in the compactness of one and the same kind of matter confers on it a difference of capacity for the reception of the electric fluid. And again, the same kind of metal, though of the same compactness throughout, will have a different electric capacity by being of different degrees of polish on different parts of its surface.* Hence, by having its opposite surfaces of different degrees of polish, those surfaces will be of different capacities for the reception of the electric fluid; and will, under the common pressure, be positive and negative with regard to each other (13).† Hence, it becomes obvious that unless a body be perfectly homogeneous in itself, and of equal polish on every part of its surface, it is physically impossible that it can be equably electrized throughout; or even of uniform electric tension on every part of its surface.

44. The electric conditions of bodies in all the cases above mentioned (41, 42, 43,), are those which they would assume under the ordinary natural electric pressure, when equably distributed on every side alike; or whilst they are perfectly surrounded by the atmospheric air, and sufficiently distant from other bodies not to be influenced by the fluid they contain: for when one body is in contact with another body, since their natural electrical conditions depend upon a circumambiency of equable pressure: that equable exposure being destroyed by the plane of contact, the bodies change their electrical character, and a new equilibrium is formed, different to that which either of them assumed separately.‡ Hence it follows that, as the materials composing the earth lean against, or rest upon, one another, the individual masses are

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* See my Experimental Researches, Part 1, p. 64, 1830.

† Ibid, p. 72; and Annals of Electricity, Magnetism, and Chemistry, &c. Vol. I. p. 11.

‡ There is no experiment more decisive on this point than that first shown by Volta with the copper and zinc discs. Before the discs have been brought into contact with each other, each metal has its share of electric fluid due to an equable circumambient electric pressure; but when their faces are in contact, the natural pressure is removed from the joining surfaces, and a portion of fluid flows from the copper to the zinc, or in the line of least resistance (40); both metals assuming thereby new electrical conditions.

seldom, if ever, exposed to an equal circumambient electric pressure, and consequently are in electric conditions accordingly to their natural dispositions (41, 42, 43,) conjointly with their connexions with one another.

45. From these considerations we are led to understand that all compound particles are electro-polar, or have their opposite sides in different electrical conditions (42). And this will be the case, whether the compound be solid, liquid, or gaseous. We learn also, that bodies even of the same kind, (instance pieces of metal) become electro-polar from a variety of causes (13, 43, 44). Bodies also become electro-polar by an unequable pressure on their opposite surfaces, though not in contact with other bodies. This kind of electro-polarity arises from a local disturbance of the natural equilibrium by a vicinal superior, or inferior electric force, located on one side only, or by the influence of both forces on opposite sides at the same time. (40. 52, 54).

46. As bodies, when in contact with each other, assume an electric equilibrium as decidedly as if perfectly insulated, (44) it follows, that the bodies composing the earth's surface and the circumambient atmosphere, would constantly observe an unvarying electric equilibrium, were no physical changes to take place amongst them. But as the whole are continually undergoing a change of temperature, corresponding electrical changes are as constantly going on; not only from the *direct* thermometric variations, but, *indirectly*, from a variety of secondary events, or physical vicissitudes in the liquid and aerial matter; by means of which the electric fluid is scarcely ever at rest, being transported from one body to another, as their capacities for its reception alter, and giving rise to other phenomena, and various modifications of matter, unproducible by any other natural agent.

47. Notwithstanding the various degrees of susceptibility which bodies present for the reception of the electric fluid, its natural elasticity, or innate repulsiveness, appears to be essential to its intromission; unless the mutual attractions between it and other matter be regarded as the only necessary qualification for its universal presence throughout nature, and its surprising volant motions through the air, and active transiliencies among the generality of terrestrial bodies.

48. Electrical attraction is an attribute well known from the earliest period of the science, and has maintained its position in almost every hypothesis hitherto framed: and appears to be sufficiently obvious and self evident in the display of some electrical phenomena, as to be understood by every observer. The opposite force, however, has had to encounter

many theoretical difficulties, which some philosophers have shown a proneness to place in its way; and is far from being generally acknowledged, as an existing principle, at the present time. Indeed, electric repulsion is totally discarded by some writers, although this force seems more essentially concerned in the production of the generality of electrical phenomena, than even attraction itself. And as some of those phenomena appear perfectly inexplicable by the attribute of attraction alone, I have not hesitated to introduce *repulsion*, as a fundamental principle, into the groundwork of the theory I have advanced, considering it as a natural attribute of the electric fluid (40). And I hope to be enabled to show, not only its efficacy in the production of phenomena, but its absolute indispensableness in the explanation of some of them.

49. The repulsion attributable to electricity has many analogies in physical science. No philosopher has yet disputed the existence of magnetic repulsion, not even those who have denied the existence of a similar force in electricity; although, in many instances, the experimental evidence is as favourable in the latter as in the former class of phenomena.

50. Elasticity is a fundamental principle in the study of Pneumatics, universally acknowledged without even a dissentient opinion. But to what attribute of its particles is the elasticity of the air traceable? Why does the remaining air, in a receiver, attenuate *itself*, and fill the whole capacity after a portion has been withdrawn by the action of the pump? And why this attenuation and expansion of even the last remaining portion of air, after the most perfect exhaustion that the pump is capable of making? These are important questions to which the term "elasticity" gives no satisfactory answer. "Elasticity is expressive of no cause, signifying only the capabilities and susceptibilities of the body to which, as a convenient term, it is applied. Elasticity depends upon an innate repulsive power, which the particles of air, or other gaseous media, naturally possess, and which is indispensably essential to their existence as elements of elastic fluids. Repulsion is, therefore, an atomic attribute, and the most remote physical cause to which the expansive force of air can be traced: whilst elasticity expresses both the tendency to expansion, and the susceptibility of compression, which extensive groups of the repulsive particles exhibit.

51. Now most writers on electricity have defined the electric fluid as highly elastic: and as we have seen that an innate repulsiveness of the elementary particles is obviously the primary physical cause of all fluid elasticity; this natural attribute is as applicable to the electric fluid as to the atmos-

fluids. Hence, as far as elasticity is concerned, there is no more impropriety in attributing the cause to an inherent atomic repulsion in the one case than in the other. And, although we do not find condensed portions of atmos-fluids repelling one another without any apparent connexion, in the manner we observe repulsions amongst positively electrized bodies, we have ample analogy in magnetism (49); and perhaps we have no reason to expect those analogies to hold good in the infinitely grosser, and comparatively inactive atmos-fluid matter.

52. I know of no fact in which electric repulsion is more obviously the cause, than the polarization of bodies by locality (45), although there be an abundance of them which bear ample testimony of its essential influence. If a positively electrized body, P, fig. 89, Plate XII., be brought to within a short distance of an insulated body, B, it is well known that the latter will become electro-polar; being negative at the extremity *n*, but positive at *p*; and neutral at about the central section, C B. Now, I would ask any philosopher who has contemplated this beautiful and highly interesting fact, with that degree of attention which it so eminently deserves, to what power he would ascribe its appearance? Certainly not to any attractions that could possibly be devised. The attraction of the body for the electric fluid at *n*, could not be lessened, nor that at *p* augmented by the approach of the body P. We do not attribute the electrical appearances to any change that has taken place in the metal, but to a *disturbance* of the electric fluid which it contains, or which resides within it, or about it. But a disturbance implies a disturbing force; which force must have been in, or about, the body P; because the phenomenon was not exhibited prior to the location of that body: nor does it continue only during the locality of the bodies P and B. Moreover, the mere metal of the body P, did not constitute the disturbing force; because if that body had been in its natural electric condition, the polarity of B would not have happened. Hence the disturbing force proceeded from some natural attribute of the electric fluid in, or about, the body P, and not from any property of the metal itself.

Now, an attraction between the fluid in P and the metal of B, could be no means of the latter becoming electro-polar in the manner shown by the experiment: nor can it be shown that such attraction would cause any disturbance of the fluid in B, unless there were an absolute introgression of fluid to B; a circumstance neither known nor supposed to take place: for when P is removed to a sufficient distance, the body B is found in its natural inert electrical condition.

53. The difficulties presented to the explanation of this species of polarity, by rejecting electric repulsion, are entirely removed by the admission of that principle, or attribute, into the theory. Let the accumulated fluid on the body P, repel the fluid in the body B, and the explanation becomes exceedingly easy and familiar. The body B, being a good conductor, its fluid would be easily put into motion by the disturbing repulsive force on P. By this repulsive force, the fluid originally occupying the extremity *n*, of the body B, would be partially dislodged, and driven towards the extremity *p*. The extremities *n* and *p* would then be respectively negative and positive, as shown by the experiment.

54. The distribution of the fluid on B, will vary accordingly to the extent of disturbance; which may be made to vary either by varying the electric power of P, or by varying the distance between the two bodies. Hence it is obvious that the position of the neutral plane B C, will change its position accordingly with these circumstances. The neutral part of B can hardly be defined as a plane in all cases; because, when the conductor B is large, there is a neutral zone of some considerable dimensions, whose position will vary with the circumstances connected with the disturbance.

55. If, instead of being located with the positively electrized body P, the conductor B, were in the vicinity of a negatively electrized body N, fig. 90, the character of polarization in B would be the reverse of that in the former case. Or, the nearest extremity *p* would be positively, and the remote extremity *n*, negatively electrical. To explain this event we have only to understand that the natural electric pressure (41) on the extremity *p* is lessened by the presence of the negative body. The fluid in B will now obey the law observable in all fluids, and will move in the line of least resistance, or, towards the extremity *p*, and thus the body B will become electro-polar. When the negative body N is sufficiently removed, the fluid of the body B will again experience an equable circumambient pressure (41), and will consequently resume its former natural distribution.

56. The experiment which I have been describing has been known to electricians for many years,* and the expla-

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* Otto Guericke, a famous philosopher, and Burgomaster of Magdeburg, who flourished about 1670, was the first to discover that bodies could be electrized without having the fluid communicated to them. Priestley's History of Electricity, p. 8. Some experiments by Stephen Grey also showed electrization by locality. This ingenious electrician, in the year 1730, electrized a boy, suspended by silken cords, by bringing near to him an excited glass tube.

nation which I have given of it, is far from being new. It is, in principle, the same as is given by several authors; but perhaps the most minutely described by Viscount Mahon, afterwards Lord Stanhope; by whom the greatest variety of experiments were made on loco-electrization that are on record.* I have repeated most of this Nobleman's experiments on loco-electrization, and have found his descriptions of the phenomena exceedingly correct; although in some others he appears to have been very much deceived. Beccaria also made many interesting enquiries in this class of phenomena,† which I shall have more particularly to notice in another place. Dr. Milner, of Maidstone, also made many exceedingly interesting experiments in loco-electrization, which are diversified in a great variety of ways.‡ I am well aware that Lord Stanhope's explanation of these phenomena has not been generally received as orthodox electricity, although it is by far the simplest and most intelligible that has been proposed: and in every other attempt at explanation the principle of *repulsion* is obviously resorted to.

57. Mr. Morgan has attempted to repudiate Viscount Mahon's theory with greater determination than, perhaps, any other writer on this subject; but his views of different phenomena are not very consistent with each other; and with reference to the present question, not very satisfactory. Whilst speaking of the polarized body, Mr. Morgan says, in rather a lofty tone, "a metallic body is said to be in two different states at the same time. What single electrical fact is there to warrant this assertion? What is there intelligible in electricity, if we admit that perfect conductors can have their equilibrium disturbed, or that two different parts of them can be at the same time in two different states, when there is no kind of insulation to separate the positive from the negative, but, on the contrary, such a communication, as in every other instance, immediately restores the equilibrium?"§ Perhaps

When the tube was brought near his feet the greatest action was about his head. Phil. Trans. Original No. 417, p. 81. Hutton's Abridgement, Vol. VII. p. 459. Priestley's History of Electricity, p. 32. But Canton, Franklin, Æpinus, and Wilcke, made the first interesting series of experiments on this branch of electricity. Priestley's History of Electricity, p. 211.

* Principles of Eletcricity. By Viscount Mahon, Quarto. 1779.

† *Giambattista Baccaria* dell' electricismo artificiale e naturale. 1753, Turin, Quarto. English Translation, sec. 3. 1776.

‡ Experiments and observations in Electricity. By Thomas Milner, M. D. 1783.

§ Morgan's Lectures on Electricity, Vol. II. p. 275. 1794.

whilst Mr. Morgan was writing this sentence he had forgotten that he had previously shown that a continuous conductor could be both positive and negative at the same time. Whilst describing the electric conditions of the coatings of one jar and the lining of another, in metallic connexion with each other, he says, "the deficiency is in connexion with the superabundance."*

58. Those electricians who have written in favour of Du Fay's hypothesis of vitreous and resinous fluids, explain the phenomenon (52) upon the principle of decomposition. The vitreous fluid of the body P, fig. 89, attracts the resinous fluid of the conductor B; and at the same time *repels* the vitreous fluid of B to the most remote extremity *p*. When the conductor B is located with a negatively electrized body N, fig. 90 (55), the resinous fluid of the body N is supposed to attract the vitreous fluid of the conductor B, and draw it towards the nearest extremity *p*; whilst, at the same time, the resinous fluid of N repels the resinous fluid of the conductor B, to the remote extremity *n*. Thus, in both cases, *repulsion* is required to explain the phenomena. The same principles are here applied to *both* fluids, as Franklin gave to his *one* fluid; and as by the latter the problem is easily solved, there can be no philosophical propriety in admitting the compound fluid of Du Fay.

59. The phenomenon which appears to present the greatest impediment to the universal reception of electrical repulsion, is the separation of light bodies when negatively electrized. The hypothesis of Du Fay admitting of electric repulsion in both its fluids, is perfectly prepared for the explanation of this phenomenon; by alluding it to the natural attribute, common to the vitreous and resinous fluids; which, although attractive of each other, are individually repulsive of themselves (58). Hence, the difficulty in solving the problem which this phenomenon presents is limited to those who admit of one electric fluid only; some of whom have had recourse to the supposition, that all matter is repulsive of itself; and that bodies which are negatively electric exhibit this supposed universal attribute more perfectly than when in any other condition. This method of explaining the phenomenon was first proposed by M. Æpinus,† and afterwards adopted by Mr. Wilcke,‡ and still further supported by the Honour-

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* Morgan's Lectures on Electricity, Vol. I. p. 110.

† Tentamen Theoriæ Electricitatis et Magnetismi, Petersburg, 1759. Priestley's History of Electricity, p. 395.

‡ Ibid. p. 396.

able Mr. Cavendish, who made an immense number of experiments, with some mathematical investigations, which to him appeared favourable to that hypothesis.* The same views of negatively electrized bodies are still favoured by some more modern writers on this subject.

60. To me, there appears no necessity to load the theory of electricity with this auxiliary force; because other methods of solving the problem are quite as satisfactory. Let the two balls, *x y*, fig. 91, be attached to a negatively electrized body N. Now the balls being deprived of their natural share of fluid, will endeavour to recover it again from the nearest portions of the surrounding air; and in consequence of both of them drawing a supply, at the same time, from that plate of air directly between them, it will become more negative than any other stratum in the vicinity of the balls, and consequently less enabled to continue the supply. This being accomplished, and the balls giving their newly acquired fluid to the body N as fast as they collect it, they will remain negatively electric. There will now be an attraction exercised between these balls and the electric fluid of the atmosphere; and as the balls yield this fluid to the body N more freely than it is capable of extricating itself from the distant particles of the air, the balls travel in quest of new supplies towards those places where it is most abundant, or *from* the stratum which they have already partially deprived of its electric fluid. Hence it is that the balls move in opposite directions from their original plane of contact, and by their divergency appear to repel one another; although, it is probable that the phenomenon does not essentially depend upon repulsion, but is principally the consequence of electrical attractions.

61. Some experiments related by Earl Stanhope have been the cause of more theoretical speculation on this particular topic, than any other with which I am acquainted; and, like some of more modern date, have been productive of very much delusion. The experiments and the inferences drawn from them, are described in the following manner:—

"*a. Experiment* 1, *fig.* 92. I took a pair of cork-ball electrometers A B, whose balls were three-eighths of an inch diameter, and whose *parallel legs* were eight inches long; and I suspended them to a hook that was fixed to the underside of the brass cap *c d* of the glass receiver E, F, G, H, of an air-pump, as shown in the figure. The two legs of this electrometer, in order to prevent their twisting, were made of fine straws which had been previously well soaked in salt and water, to make them conduct better. Each of these legs was suspended at top, by a very fine linen thread of about *one-twelfth* of an inch in length.

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* Phil. Trans. Hutton's Abridgement, Vol. XIII. p. 223.

" *b.* In order to render the glass receiver a good *non-conductor* of electricity, I caused it to be perfectly well dried by means of fire. It was proper, in this experiment, to avoid giving any friction to the glass receiver, for fear of charging the glass.

" *c.* Upon charging a small prime conductor, which was made to communicate with the brass cap of the glass receiver, the electrometrical balls divaricated above *two inches and a half.*

" *d. Experiment* 2. I then began to *exhaust* the receiver; upon doing which, the balls soon began to devaricate *less and less.* And as soon as the short barometer gage was got down to about *one quarter* of an inch (the barometer being, that day, at the height of *twenty-nine inches and a quarter*); the devarication of the balls from each other, became reduced to *less* than *one quarter* of an inch.

" *e.* So that, by $\frac{116}{117}$ parts of the natural quantity of air contained in the receiver, being *exhausted,* the devarication of the electrometrical balls was diminished to less than *one tenth* part. For, the *chord* of the angle of divarication was decreased (as was said before, *c* and *d*) from above *two inches and a half* to less than *one quarter* of an inch. That is to say, that the *versed sine* of the angle of devarication, was decreased considerably more than an *hundred times*; because, *the versed sines are always as the squares of the chords.*

"*f.* I should be inclined to imagine, if this experiment were made with great accuracy, and with a proper electrometer, that the *versed sine* of the angle of devarication, would always be in the *same ratio,* as the density of the air in the receiver; provided that proper means were taken to keep the apparatus sufficiently free from moisture during the experiment.

"*g. Experiment* 3. I then electrified the glass of the receiver itself; but as long as the receiver remained *exhausted* to the degree above mentioned (*d*), it was out of my power to cause the electrometrical balls to devaricate above *one quarter* of an inch.

" *h. Experiment* 4. I then let the air return into the receiver, which circumstance alone caused the balls again to divaricate considerably, although I gave to the apparatus *no fresh supply of electricity.* But the balls took up a short time in coming to their full degree of devarication. The reason of which, evidently was, that the *unelectrified* air, which entered into the receiver, could not receive *immediately*, from the charged apparatus, that degree of electricity which it was able finally to acquire.

"*i.* I repeated these experiments with the exhausting air pump, several times, and I always found results that were similar.

"*j.* From these experiments, it appears, that, when bodies are charged with electricity, it is the *particles of (circumambient) air being electrified*, that constitutes the *electric atmosphere* which exists around those bodies.

"*k.* Now, since an *electrical atmosphere* (whether *negative* or *positive*), consists of electrified *air*, it evidently follows, that the density of the electricity of the air, must be in some *inverse ratio of the distance* from the charged body, which causes that electric atmosphere.

"*l.* That electric atmospheres do decrease *in density*, the more the distance from the electrified body is increased, is demonstrable by means of a proper electrometer in every instance.

"*m.* From these simple considerations, it is easy to reduce all the different *phenomena* of electrical *attraction* and *repulsion* to *one* plain and convenient principle, derived from the very nature of a *disturbed electrical equilibrium* ; namely, to the elastic tendency of the electric fluid, to impel every body, charged either *in plus* or *in minus*, towards *that part* of its electric atmosphere, where its *natural electrical equilibrium* would be the most easily restored.

"*n.* From this simple principle, it is evident, that bodies, which are charged with contrary electricities, must tend to *approach* each other, whenever the skirts of their (oppositely electrified) *atmospheres* interfere.

"*o.* From the same simple principle, it is also easy to understand, why bodies, that are charged with the *same kind* of electricity, tend to diverge from each other.

"Every body that is electrified (whether *in plus* or *in minus*) has a constant tendency to return to its *natural state ;* and this causes it to electrify, in a certain degree, *other bodies* in contact with it, and the *air* in its vicinity, in a manner similar to that explained above.

"*p.* If two bodies (for example) be both *positive ;* neither body will be able to deposit its *superabundant* electricity upon the other body, which is also similarly electrified *in plus.* It is therefore evident, from the simple principle mentioned above (*m*), that if these bodies be brought near each other, *each body* will be impelled, towards the particles of air on its *other* side, which are electrified *in plus* only in a *small* degree. That is to say, that each body will tend to *diverge* from the other.

"*q*. If these bodies, on the contrary, be both *negative*; neither body will be able to have its *deficient* electricity supplied from the other body, which is also similarly electrified *in minus*. It is therefore evident, from the the simple principle mentioned above (*m*), that, if these bodies be brought near each other, *each body* will be impelled, towards the particles of air on its *other* side, which are electrified *in minus* only in a *small* degree. That is to say, that each body will tend to *diverge* from the other.

"*r*. So that, bodies, which are charged with the *same kind* of electricity (whether *positive* or *negative*), must necessarily tend to *diverge* from each other."

62. The inferences which the noble author has drawn from his experiments are obviously at variance with the doctrine of electrical repulsion; which is the more remarkable because he has acknowledged the *elasticity* of the electric fluid (*m*); a property evidently traceable to the attribute of repulsion exercised by its individual particles (50, 51).

63. There can be no objections to Lord Stanhope's method of explaining the phenomena, as far as it proceeds; because attraction is unquestionably in play in the divergency of similarly electrized bodies, as that phenomenon is usually displayed: but neither the experiments of that nobleman, nor the inferences he has drawn from them, have any tendency whatever to disprove the existence of a repulsive electric force.

64. I am well aware that these experiments are usually looked upon as master-pieces of their kind, and are much admired and frequently quoted by those philosophers whose opinions are hostile to the doctrine of electrical repulsion; and as their correctness has never yet been disputed, they are regarded as affording *standard* data, on which much theoretical speculation has been founded.

65. To me, however, these experiments have never appeared in that light; but, on the contrary, I have always considered the data which they afford, much too scanty, if even the *recorded* results had been admissible as facts on which implicit confidence could have been placed. And on looking at the circumstances connected with the experiments, it is not difficult to perceive that those results are placed in a very questionable posture; and are obviously objectionable in whatever point of view the scientific electrician may contemplate them.

66. The electrometer (60. *a*.), which Lord Stanhope em-

ployed in these experiments, was not much calculated to give very exact results in an atmosphere so far attenuated as that it is said to have been placed in (60. *d.*). Its balls were far too heavy (60. *a.*) to be kept divergent by any electric force which they could retain in so good a conducting medium as that of an atmosphere supporting only one quarter of an inch of mercury. The electric force which kept the balls two inches and a quarter apart, (60. *c.*) in a common atmosphere, would be mostly lost in the attenuated one, (60. *d.*); for withdrawing the air with the pump, would remove the insulation; and consequently a portion of the fluid would make its escape from the straws and their balls; probably to the base of the instrument. But it is stated by Lord Stanhope that the electric force was *not* lost by the attenuation of the air; for he says that when the air was readmitted, the balls again devaricated "considerably, although I gave to the apparatus *no fresh supply of electricity*" (60. *h.*): and from the subsequent part of that paragraph we are led to understand that the balls ultimately diverged to the same extent as at first; a conclusion not very consistent with the doctrine of electric atmospheres (60. *k.* to *r.*). For, one would be led to suppose that the first electric atmospheres of the balls would be partly removed by the action of the pump, and another pair have to be formed on the readmission of the air, and the formation of these second atmospheres would have to be at the expense of the electric fluid from the balls, straws, and cap of the instrument, in all of which, the remaining fluid would be attenuated, and the divergency ought not to be so great as before.

67. I do not know that Lord Stanhope's experiments have ever been repeated; or if they have, I should suppose, from the estimation in which they are held by some of our latest writers on this subject, that they have never been much varied; and but very imperfectly understood. Singer, who has left the best treatise on electricity in the English language, speaks of Lord Stanhope's experiments with great confidence: and quotes them in support of his opinion of there being no such attribute as electric repulsion.* Since the publication of Singer's work, I cannot find that any scientific journal has noticed these celebrated experiments farther than an occasional quotation; and therefore I am in hopes that the experiments which I am about to detail, will appear interesting, not only to the London Electrical Society, but to philosophers

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* Elements of Electricity and Electro-chemistry. By George John Singer, p. 24.—London, 1814.

generally who are engaged in theoretical enquiries in this branch of physics.

68. I prepared an electrometer similar to that employed by Lord Stanhope (60. *a*.); and proceeded to repeat the experiments in the manner already described (60. *c*, *d*, *h*,). Prior to the attenuation of the air in the receiver, the cork balls were caused to diverge to about two inches from each other by the application of an excited glass tube. The pump was immediately brought into requisition, and whilst the air in the receiver was being attenuated, the divergency of the cork balls began to be lessened; and before the attenuated air was counterbalanced by one inch of mercury, the balls got to within a quarter of an inch of each other. The air was now readmitted to the receiver very slowly; but the balls showed no tendency to separate again. I repeated this experiment many times with similar results.

69. It now occurred to me that the cork balls were too heavy; and that, relatively to the two *extreme* conditions of the air (dense and rarefied) in which they were immersed, they would be heavier in one case than the other. This latter circumstance, however, could not prevent the balls from diverging again when the air was readmitted; for if the electric force had continued unimpaired till the return of the air, the divergency ought to have progressed as the density was restored.

70. I now removed the cork balls from the ends of the straws, and replaced them by small balls of the pith of the elder. I again electrized the apparatus with an excited glass tube until the balls diverged to about two inches from each other. On attenuating the air to the same extent as before, the balls approached to within about 3-8ths of an inch of each other. The air was readmitted gently, but the balls never separated any farther than whilst the air was attenuated. This experiment I also repeated several times and the results were always of a similar description; the collapsion of the balls usually bringing them to *within* half an inch from each other when the air to which they were exposed counterbalanced one inch of mercury; but in no case did they diverge again when the air was restored to its usual density. From these facts nothing seemed more likely than that a portion of the electric fluid had escaped by the attenuation of the air in the receiver.

71. My next experiment was intended to ascertain if the pith balls could be made to diverge to two inches in air attenuated so as to counterbalance only one inch of mercury. The air in the receiver having been brought to this

standard density, the excited glass tube was brought to the cap of the instrument. The straws and balls now exhibited some strange antic motions not easily described. They would first suddenly diverge to a considerable extent, and as suddenly return to their vertical position in the axis of the glass; repeating these motions two or three times before the tube came into actual contact with the cap of the electrometer. And it was often with great difficulty that they could be made to remain separate when the excited tube was taken away. From the results of this experiment, often repeated, it seemed obvious that the fluid was given off by the straws and balls, either to the sides of the receiver, or to the pump plate. The latter, however, appearing the more probable course for it to take, I contrived the following experiment to ascertain how far this view might be correct.

72. Two electroscopes were provided for this experiment, which, when properly prepared, were placed the one on the other, as represented by fig. 93. The receiver A, prior to its being situated on B as in the figure, was placed on the plate of a jet d'eau experiment apparatus, whilst the latter was screwed in the orifice of the pump plate. The air in A was then attenuated until it would just counterbalance one inch of mercury. This done, the communication was cut off by turning the stop-cock *s*, the apparatus was then taken from the pump, and screwed to its base *d*, and afterwards placed on the electroscope B, whose contained air was of the density of the atmosphere. On bringing the excited glass tube near to the cap of the electroscope A, the pith balls made several singular motions, but did not evince much tendency to diverge from each other. But the pith balls within B, diverged to an inch and a half at least, as decidedly as if the tube had been brought *directly* to its cap. When the excited tube was taken away, the balls in A hung close together; but those in B remained divergent, as shown by the figure. The tube was excited anew, and again applied to the cap of A, whose balls again were much agitated, and the balls within B diverged farther than before, and even struck the sides of the glass. By a few trials I found that any degree of divergency might be given to the balls in B; but that there was great difficulty in keeping those in A separated more than about half an inch from each other after the excited tube was taken away: although, by regulating the distance between the tube and the cap of the instrument, they might be made to separate one inch or more.

73. The experiment (72) was repeated, by applying a large piece of excited amber, instead of the glass tube, to the

cap of the instrument A. The results were similar to those obtained when the glass tube was employed, only the balls in A were not so much agitated. The balls in B, separated as before, though not to so great an extent; whilst those in A, separated farther than by the application of the tube; and remained more divergent after the amber had been taken away. The balls in B were made to separate two inches and a half, by three applications of the excited amber to the cap of A; and remained separated for some time, after the amber was removed.

74. Now, although the results of these experiments appeared satisfactory enough that the fluid communicated to the instrument A, from the glass tube, was transmitted through the attenuated air to the lower instrument B; and when the amber was used the fluid moved in the opposite direction: yet it was necessary to ascertain how far the two instruments would be affected by electrical locality alone (52), when both were filled with air of the common density of the atmosphere, which at that time counterbalanced 29·6 inches of mercury; the temperature of the room being 60° F. The two instruments were again placed as in fig. 94, and the excited glass tube made to approach the cap of A. The balls in this instrument diverged considerably; but those in B were scarcely affected. On bringing the tube into contact with the cap of A, the pith balls struck the side of the receiver; and those in B, separated about half an inch; and remained about a quarter of an inch apart after the excited tube had been taken away. The experiment was often repeated and with similar results: excepting that the balls in B did not *always* remain divergent after the excited body was removed from the cap of A: but generally remained as in the figure.

75. By comparing the results of the above described experiments (71, 72, 73, 74,) we discover a material difference between those which were produced when the air in the instrument A, was attenuated, and when at the common density of the atmosphere. When the balls were immersed in the attenuated air (71, 72, 73,), their motions were rapid and exceedingly irregular; unless great care were observed in bringing the excited body very slowly towards the cap of the instrument. And even when the greatest care was taken, the balls would suddenly strike each other after a moment's separation: and would repeat these motions two, three, and often four times before the excited tube arrived at the cap of the instrument. But when the air within the instrument A, was of the common atmospheric density, no such vacillancy was exhibited by the balls (74). The divergency was inva-

riably regular, and progressive as the excited body approached the cap of the instrument: and in no case did the balls strike each other whilst under the electric influence of the tube, or the amber. Another striking contrast in the results of these experiments is observable in the *ultimate maximum* divergency* of the balls in the two instruments. When the air is attenuated in A, the *ultimate maximum* divergency is invariably *greater* in B than in A (72). But when the air in both instruments is of the common atmospheric density, the *ultimate maximum* divergency is uniformly *greater* in A than in B (74). By taking into consideration every circumstance connected with the above experiments, there was every reason to suppose that when the air was attenuated in the instrument A, it became a sufficiently good conductor to carry off a portion of the fluid from the cap, straws, and balls of the instrument: and that this fluid was transmitted to the lower instrument B.

76. Thinking that, if, in place of the salted straws, two better conducting stems were suspended in the instrument A, the loss of fluid in an attenuated medium would be better observed, I procured, for this purpose, some fine copper wire, from which proper lengths were taken. One extremity of each piece was bent into the shape of a hook, and the other extremity furnished with a pith ball; and both wires were hung in the axis of the receiver. The instrument was now placed on the plate of the air pump; and before any attenuation was carried on, the excited glass tube was made to approach the cap of the electroscope. The balls separated from each other as gradually as when attached to the straws in the former experiments, in air of the common atmospheric density: and when the tube was brought close to the cap of the instrument, the balls struck the sides of the receiver. When the electric force of the tube was not too great, the divergency might be extended to any required degree without the balls striking the receiver: and if the cap were touched by the tube, the balls would remain two inches apart for several minutes after the tube was taken away. To ascertain this latter fact, was the principal object of this experiment.

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* "*Ultimate maximum* divergency" is intended to express the divergencies in their last stages; or when the excited body is withdrawn from the instrument A, which is the only period of the experiment in which a just estimate of their relative extent of divergencies can be formed. For, although the extent of divergency of the balls in B may very easily be ascertained in any stage of the process, the sudden vacillancy of those in A, precludes the possibility of knowing to what extent their earliest divergencies are carried.

77. The instrument being deprived of its electricity acquired in the last experiment, the pump was brought into play until the attenuated air in the receiver would just counterbalance one inch of mercury. On approaching the cap of the receiver with the excited glass tube, the wires with their balls were strangely disturbed: their motions being more rapid and frequent than those exhibited by the straws; but remained close together when the tube was taken away. By a few trials, I got into the method of leaving the balls separated about half an inch from each other; but in no instance could I obtain an *ultimate maximum* divergency to a greater extent: and even this only for a few moments after the excited body was withdrawn, for the balls soon came down to less than a quarter of an inch from each other.

78. I now varied the experiment by first electrizing the wires and balls, and afterwards attenuating the air, as had been done whilst the straws were suspended in the receiver (68, 70). The instrument being placed on the pump plate, the balls were made to diverge by the application of the excited glass tube. When the tube was removed from the cap of the receiver, the balls were about two inches apart. The pump was now brought into play, and as the attenuation of the air proceeded, the balls came closer together; and when the mercury in the gage was reduced to one inch, the balls were less than one quarter of an inch apart. The air was readmitted very gently, but the balls never separated farther than when in the attenuated air.

79. I have repeated every experiment herein described, many times over, and have taken every care that I could think of to prevent error in the results. The experiments described (in 68, 70, 78) are those alone which can be considered as repetitions of that on which so much theorizing speculation has been ventured; and as the experiment requires no very refined experimental dexterity, it is not difficult to discover that there is some unaccountable error in Lord Stanhope's description of the results (60, *h*); for in no instance have I yet seen the least tendency to divergency of the balls by readmitting the air into the receiver; nor indeed can I see any cause for the appearance of such a phenomenon, unless it were from the greater degree of buoyancy which the balls would experience in the dense than in the rarefied air.

80. The experiments detailed (in 71, 72, 73, 74, 77) may be regarded as perfectly original, and such as the nature of the enquiry obviously required. In every one of these there has appeared, almost, indubitable proof of a *loss* of fluid through the medium of the attenuated air; a conclusion

which will be strongly corroborated by the next described experiments.

81. The electroscope, with its wire indices and balls, was placed on the pump plate, and the balls made to diverge, sometimes by the application of the excited glass tube, and at others, by the excited amber; the air in the receiver not being molested by the pump. The standard divergency was two inches, when the exciting body had been taken away. The object of the experiment was to ascertain whether the balls, when electrized to the same extent, would retain their divergency for a longer period when the air was undisturbed, or when it was attenuated by the pump. The temperature of the air of the room in which these experiments were made was 60° F., and the barometer stood at 29·6 inches. The results of the experiments are shown by the following table. The left-hand column shows the standard distance of the balls at the commencement of each observation; and the character of the excited body employed. The second column shows the time required for the loss of the standard quantity of electric action, when the balls remained in an atmosphere counterbalancing 29·6 inches of mercury. And the third or right hand column shows the time required for the same loss of electric action when the air about the balls was reduced to a pressure equal to that of one inch of mercury.

Table of experiments exhibiting the time in which an electrometer lost a standard quantity of electric action, in aerial media of different densities.

The standard repulsive distance of the pith balls, 2 inches, when excited by	Time required for the total loss of the standard quantity of electric action.	
	In air balancing 29·6 inches of mercury.	In air balancing 1 inch of mercury.
Glass	5 minutes.	1½ minutes.
Amber	4 minutes.	1¼ minutes.

82. Having now satisfied myself that the *lessening* of the divergency of the balls when the air in the receiver became attenuated (60. *d*, 68, 70, 71, 72, 73, 77, 78, 81), was owing

to a real loss of the electric fluid which they sustained; and that the total disappearance of the standard quantity was much facilitated when the air in which the balls were immersed was attenuated (81), I now thought it possible that the total disappearance of the electric action on the balls might be effected in a still less period of time than that shown in the table (81), by *diluting* the electric fluid, with which the instrument was charged, with fresh portions of air. For this purpose the receiver was made quite dry and warm: and when placed on the pump plate an electric charge was given to the balls from the excited glass tube; which caused *an ultimate maximum* divergency of two inches and a quarter. This being accomplished, the apparatus was permitted to remain, unmolested, till the divergency entirely disappeared: which occupied *six minutes and a quarter*. The instrument was now charged again to the same standard of divergency as before. As soon as the glass tube could be got out of the hand, the pump was brought into play, and the mercurial gage brought down to three inches. The air was readmitted; and again pumped out until the balls came close together. The air was now again readmitted very slowly, but the balls did not diverge again: so that by this one *dilution* of the electric fluid, the whole of its action on the balls entirely disappeared. The time occupied was *forty-seven seconds*.

83. It had occurred to me, at various times during these experiments, that there was a probability of even *seeing* the electric fluid make its escape from the balls through the attenuated air in the receiver, provided the room were darkened; but, being at that time otherwise engaged in the evenings, a considerable period elapsed before I had an opportunity of ascertaining the correctness or incorrectness of this idea. Eventually, however, the experiment was made, and with the anticipated success. The electroscope with the wire indices (76) was placed on the pump plate, and the air within attenuated till it would just counterbalance one inch of mercury. The glass tube was then excited and brought towards the cap of the instrument: and the wires and balls were agitated in the manner already described (77). The room was now darkened and the tube again excited; and then brought to the cap of the electroscope. Sparks immediately appeared from both balls, darting in a very beautiful manner to the sides of the receiver at nearly the same height as the balls were suspended: and from these places exhibited luminous streaks down the sides of the glass to the plate of the pump. This beautiful and conclusive experiment I was induced to repeat many times: during which I frequently observed three

and sometimes four of these streaks of electrical light by one application of the excited tube. The streaks of light exhibited in this experiment, are tolerably represented by the crooked lines *o b*, *o b*, *o c*; *o c*, in fig. 95.

84. When excited amber was employed instead of the glass tube, the light was seen in the receiver as decidedly as in the last experiment (83); but its appearance was very different, being much fainter and not in such well defined lines. Both of the pith balls appeared beautifully illuminated, especially on their lower sides. I tried to vary the light by placing pointed wires on the pump plate, as represented by *w w*, fig. 96. By this means the light about the balls became a little brighter than before, and extended farther from them, always inclining towards the points *w w*; but in no instance was there much brilliancy, nor any defined streaks of light, as when the glass tube was used. The figure of each portion of light had some resemblance to an inverted cone, as represented at *b c*, fig. 96, and did not appear very unlike the tail which some comets have exhibited.

85. It may here be proper to state, that I have invariably obtained more accurate results from the metallic wire indices, or stems (76), of the electroscope, than from the straws soaked in a solution of common salt (60 *a*). With the former, the loss of fluid is regular, and uniformly the same in every experiment made under similar circumstances: but with the latter the results are much influenced by the hygrometric condition of the salt, and also by the asperous surfaces which it gives to the straws. Dry straws which are not salted give different results to those which are so treated: and gold leaves give still different results to either straws or wires. These and many other circumstances connected with electrometric experiments in air of different degrees of density, and some novel facts, will be more particularly noticed in my second memoir, which will shortly be submitted to the consideration of the Electrical Society. As far as I have hitherto proceeded, there have not appeared any facts which militate against the operations of a repulsive power in the display of electrical phenomena: but on the contrary, there is much evidence in favour of the existence of that attribute. It will have been observed that Lord Stanhope's experiments are totally inconclusive on this point: and it is somewhat remarkable that his Lordship, who had made so many excellent experiments on the polarization of bodies by electrical locality, should, in this instance, have confided so much on one single experiment, without even the slightest variation: for his Lordship says, in the appendix to his work, at page 235, that those "experiments were performed

with *positive* electricity only." And again at page 236, "It was quite unnecessary, to make any similar experiment with *negative* electricity." From these statements and from the great confidence which Lord Stanhope placed on this single experiment, it is obvious that his Lordship had not the slightest suspicion of the escape of fluid in the attenuated air: and from the implicit sanction which this experiment has generally met with amongst philosophers, it would seem that not the remotest idea of the fact has hitherto been entertained.

86. It will have been observed that in none of the experiments hitherto described have I attenuated the air in the receiver to a greater extent than as a counterbalance to one inch of mercury: whereas, in Lord Stanhope's experiment, the density of the air was reduced so as to counterbalance only one quarter of an inch of mercury, (60. *d.*). I have adhered to the former standard density of the air in the receiver on two different accounts. First, because when the mercury in the *short* barometer gage had fallen to one inch, the cork balls had approached to about a quarter of an inch of each other (68); which is the *shortest* distance between the cork balls in his Lordship's experiments (60. *d.*). Secondly, as the generality of pumps which are in the hands of experimenters, will reduce the mercurial column to one inch, and but only a few of them sufficiently accurate to bring it down to a quarter of an inch, I have thought it better to record the experiments under those circumstances in which they may be repeated with the greatest facility; and within the range of those means which are at command by the greatest number of experimenters. It will be necessary to mention, however, that I have attenuated the air about the electrized balls to a much greater extent: but, as might have been expected, the loss of the fluid was greater as the mercurial column, in the gage, shortened: and I have often found, that, when the air was counterbalanced by less than half an inch of mercury, the balls would come down to one tenth of an inch from each other. Much exactitude, however, will always be required in experiments of this kind; for, as the loss of the electric fluid will depend both on the *attenuation* of the air and on the *time* occupied in pumping (81. 82.), it is obvious that the divergency will be lessened on both these accounts, and the *distance* between the balls when the mercurial column is reduced to any *standard altitude*, will depend upon the *time* occupied in bringing the air to the given degree of attenuation.

87. The *time* required to bring the mercurial column of the gage down to a quarter of an inch, is very great when compared with that necessary to reduce it only to one inch;

even when the best pumps are used. In the experiments I have described, the *time* of pumping was particularly attended to; the standard being *thirty seconds:* which, under those circumstances necessary to guard against the agitation of the apparatus, was the shortest period that the pump which I employed would allow to bring down the mercury to one inch. Under all these circumstances it is obvious that much caution is necessary whilst carrying on experiments of this delicate nature; and that the standard attenuation of air, which I have employed, is much better calculated to give exact results, than when the attenuation is carried on much farther. And as the propagation of novel facts is always facilitated by simplifying the means of producing them, I have been anxious to place these within the reach of every experimenter: hoping they will be the means of removing some of those theoretical prejudices which have so long rested on the *report* of one solitary experiment.

Westmoreland Cottage,
Nov. 26th, 1837.

LXV. *Facts and Observations for the purpose of illustrating a Theory intended to connect the Operations of Nature, upon general principles.* *By* PAUL COOPER, ESQ.

(*Continued from page* 374.)

78. This experiment has the advantage of presenting local circuits in a form in which they cannot be mistaken. When zinc is dissolved in diluted acid, the positive and negative ends of the circuits are not distinguished from each other by any striking difference in their appearance; for although the oxidation takes place at a negative surface of the metal, and the hydrogen is evolved at a positive surface, where there is no oxidation, the different circuits are so mixed together, and the operations are upon such a minute scale, that we cannot distinguish the one from the other. But here, lead being separated instead of hydrogen at the positive surface, and the atom last deposited being the foundation for the next in succession, in consequence of its forming the positive termination of the circuit, while the oxidation of the zinc proceeds at the negative termination, as with the diluted acid, there can be no doubt of the existence of these circuits, or of their character and utility. The two operations in which an atom of oxygen is simultaneously taken from the lead and added to the zinc, at points considerably distant from each other,

could not take place with such undeviating regularity if these operations were not connected, so as to be dependant upon each other.

79. The electro-motive force in this experiment arises from a difference in the electrical forces of lead and zinc. There is an atom of zinc in contact with oxygen at one end of the circuit, and an atom of lead in contact with oxygen at the other end of the circuit, the intermediate space being occupied with hydrogen and oxygen in alternate atoms, (as in fig. 81, Plate XI.); and as the current is from the zinc, it is a proof that it presents to the oxygen a surface more negative than that which is presented to it by the lead at the opposite end of the circuit (54). In confirmation of this, when the oxygen is united to the zinc and the lead reduced to a metallic state, the circumstances are reversed; but there is no reverse current; the zinc, notwithstanding its closer connexion with the oxygen, having still the most negative surface. It is quite necessary, however, to confine ourselves to the surfaces of the metals in the state which is induced by the combination in which they are here presented; a different combination might produce different results, and the natural electrical forces of the two metals are known to be the reverse (80).

80. These investigations are extremely curious and interesting; and they open a field of enquiry which leads to the most unexpected results. In the present instance we arrive at a conclusion in direct opposition to our previously conceived notions with regard to the affinities of positive and negative bodies; but which is completely in accordance with facts derived from experiment. In the early stages of this enquiry I came to what appeared to me the very natural conclusion, that the electro-motive force depended upon the greater attraction of the positive element for the new negative element, than the latter had for that which it quitted to make the exchange (153); but subsequent investigations have convinced me that this conclusion is altogether erroneous, and that the real state of the case is, in a great measure if not entirely, the reverse. In the experiment before us, lead is a more negative body than zinc, and if its attraction for oxygen could be measured by the actual force which it would require to separate them, there can be little doubt that it would be found to possess the strongest cohesive force; yet, zinc takes oxygen from lead, but lead cannot take oxygen from zinc. Hence it is, that elements which have the greatest attraction for each other are the most easily separated by galvanic action.* The

* See Becquerel. Annals, Vol. I. p. 402.

most positive elements of the negative class, although they have the weakest attraction for positive bodies, produce the most powerful electro-motive forces; and upon the approach of these, by what appears almost a magic power, the stronger cohesive forces give place to the weaker. It is evident, therefore, that in estimating the electro-motive force, the whole of the surfaces included in the circuit must be taken into consideration (56).

81. We cannot suppose that powers of this description, though here applied to the production of a philosophical toy, are not intended for much higher purposes. We may in all probability proceed by a very easy transition from this experiment to the means which nature has employed in various important operations connected with animal and vegetable physiology. The food of plants may be supplied, their roots extended, and the sap which is formed kept in circulation, by electrical currents, upon the same principles that filaments of lead are formed upon the zinc, with only trifling variations to adapt them to the change of circumstances in which they are called into operation. The tap and other principal roots, perhaps, present the negative surfaces, from which circuits are formed in the surrounding moistened earth, terminating, first upon positive points upon the same roots where the negative elements are deposited; and then, by superposition, upon the first deposits, until the elongation of these points become visible in the form of fibrous roots, the atoms last deposited being the positive terminations of the external parts of the respective circuits: while similar elongations of the negative surfaces, from the deposition of the positive elements, form their negative terminations. The fibrous elongations of both surfaces are probably so formed as to admit, through capillary apertures, such portions of the different elements as are required for the growth and support of the plant, which, by forming continuations of the circuits must meet, and cause the circulation of the sap in a state of perpetual decomposition and recomposition, as parts of these circuits; and of course in a state to surrender its elements freely wherever they may be required. The lowest orders in the scale of animal existence are probably supported upon similar principles; and animal secretions generally, and in some instances their circulation, are perhaps also dependant upon them.

82. The electro-motive force which leads to the formation of these circuits, connected as they probably are with operations of such importance, must be a subject of investigation entitled to our best attention. In order to give a clearer conception of the nature and character of this force we will

return to our former experiment (68, 69), which has reference to fig. 78, Plate XI. It is evident that we may render the subject more simple by bringing the circuit described in this figure to the form of fig. 82; in which the negative surface of the copper is in immediate contact with the positive surface of the zinc, and the interposition of the connecting wire thereby rendered unnecessary. This may be done practically by forming the metallic part of the circuit of zinc and copper wires.

83. Now, it being evident that the electro-motive force depends upon the difference of the electrical forces of the bodies in contact, and that it increases with an increase of that difference (59), I have assumed, that the electro-motive force of zinc to oxygen is 5, and that the electro-motive force of hydrogen to oxygen, hydrogen being more negative than zinc, is 6. This would evidently give a balance of force in favour of a current from the hydrogen to the oxygen, and from the oxygen to the zinc, or in a direction contrary to that indicated by the arrows, which, instead of producing the decomposition of the water, would have a tendency to increase the polar forces by which it retains its present arrangement. But hydrogen is negative to zinc, and there is, consequently, an electro-motive force from the former to the latter, through the medium of the copper; this force, which I have assumed to be 2, being in the same direction as the electro-motive force from the zinc, will turn the scale in favour of a current in the direction of the arrows; and which, by reversing the poles of the atoms of oxygen and hydrogen, will necessarily produce the chemical effects already described. The direction of the electro-motive forces from the different surfaces, is indicated in the figure by the arrow heads, and the amount of the force from each by the figures placed over them. Conceiving the electrical forces of hydrogen and copper to be nearly equal (89), I have taken the electro-motive force between these two bodies as nothing; and have thus left the electro-motive force of 2, which was supposed to be due to the difference of the electrical forces of hydrogen and zinc, for the electro-motive force from the copper to the zinc. Any little difference of force between the hydrogen and the copper will make no difference in the final result; because if either should be found to be in a slight degree negative to the other, the electro-motive force arising from it will be counteracted by a corresponding increase or decrease of force between the copper and the zinc.

84. We may, however, dispose of the difficulty arising from the interposition of a fourth body in two other ways. In the first way, by referring to the local circuits which are

formed when zinc is exposed to the action of acidulated water, already described (73); in this case the circuits are formed as in fig. 82, but without the interposition of the copper; and the current is from the zinc to the oxygen, from the oxygen to the hydrogen, and from the hydrogen directly to the zinc: the chemical and other effects being in every respect the same as when copper is introduced as one of the elements of the circuit. Or, in the second way, we may suppose the interval between the plates, of which sections are given in fig. 78, to be charged with a solution of sulphate of copper: in this case, if the action upon the solution be a secondary result (77 and note), as in fig. 83, we dispose of the hydrogen by placing it between two atoms of oxygen, the equal electro-motive forces towards which balance each other, and the force of the current is derived from the electro-motive forces of the zinc to the oxygen and the copper to the zinc, minus, the electro-motive force of the copper to the oxygen. If, then, we consider the electrical forces of copper and hydrogen to be equal, as before, we shall obtain from this solution a current of equal force with that produced by acidulated water. By one of these methods we get rid of the copper, and by the other of the hydrogen; and by both we reduce the consideration of the subject to the smallest number of bodies, with difference of electrical forces, which can produce a current, with decomposition, by their action upon each other.

85. It has been found by experiment that the forces of the currents produced by these two methods, estimating them by their powers of decomposing other bodies placed in the circuits, are nearly equal; but this is not a sufficient proof that copper and hydrogen have equal electrical forces; because we have no means of showing that the amount of disposable force bears any exact proportion to the difference of the electrical forces in the bodies which produce it. For instance, we have no means of proving that, supposing the electro-motive forces of hydrogen and copper to oxygen to be equal, the electro-motive forces of hydrogen and copper to zinc must also be equal; and it is evident that, in the absence of this proof, the amount of disposable force in the currents is not sufficient evidence of the relative electrical state of the bodies which produce them.

86. But I think we can show by other experiments, that hydrogen, like copper, occupies an intermediate station, with regard to its electrical force, between zinc and platina. It has been already stated (73), that when zinc is plunged into an acid diluted with water, local circuits are formed, in which

the oxygen of the water is connected with the metal where it exhibits a negative surface, and its hydrogen with a part of the same metal, where, probably from some admixture of other metals, it exhibits a positive surface, as in fig. 84.

87. The balance of electro-motive force in this case is in favour of a current in the direction of the arrow, which must reverse the poles of the atoms of oxygen and hydrogen, and produce the chemical operations already described. Let us now substitute for the zinc a piece of platina, as in fig. 85. This metal being negative to zinc, the electro-motive force of which to oxygen we have assumed to be 5, and to copper, the electro-motive force of which we have assumed to be 6, we will assume its electro-motive force to oxygen to be 7, as in the figure. But if we take the electro-motive force of platina to oxygen at more than the electro-motive force of hydrogen to oxygen, we must necessarily conclude that platina is negative to hydrogen, and that there will be an electro-motive force from the former to the latter; this we have taken in correspondence with a former assumption (83), at 2. The balance of the electro-motive forces will consequently, in this experiment, be the reverse of the former, as indicated by the arrow in fig. 85; but a current in this direction, which must be very trifling, will only tend to increase the polar forces by which the particles of water are held in their present form, and it can therefore produce no chemical action. This we find to be the case in practice, and so far we may conclude that our theory is conformable to experiment.

88. If instead of exposing the piece of platina to the action of acidulated water, we plunge it into a mixture of oxygen and hydrogen gases, the same circuits will be formed by alternate atoms of these gases, brought into this order by their affinity for each other; and the polar forces will be induced which are indicated by the symbols attached to the different atoms in the figure, by their action upon each other. These forces, however, are not sufficiently powerful to produce a cohesive union under ordinary circumstances; but the electro-motive force of the current in the direction of the arrow, which could only strengthen the union of the atoms of oxygen and hydrogen already reduced to a fluid state, (87) will now increase the polar forces of these atoms and enable them to form particles of water by a cohesive union. Here then we again find theory conformable to experiment; the union of these gases having been produced by the means we have described, in the experiments of Dr. Faraday.* The circuits are represented

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* Experimental Researches in Electricity, Sixth Series.

in figs. 84 and 85 as terminating on opposite surfaces of the metal, but this is not by any means a necessary condition; they may terminate on the same side or on different sides, at points distant from each other, the current passing from one termination to the other through the metal.

89. As all the metals which are negative to copper, refuse, like platina, to become oxidized by the decomposition of water, and as all those which are positive to it readily submit to this operation, while copper itself, as it ought in this case, belongs to the former class, and as it has been shown that the question with regard to chemical action depends upon the relative electrical state of the metal exposed to it, compared with hydrogen, we feel ourselves justified in the assumption we have made, that the electrical forces of hydrogen and copper are nearly equal. It will be observed, that in experiments of the latter class, in which the oxygen is supplied by water, the acid, which gives activity to the operation by dissolving the oxide, is not decomposed, and consequently forms no part of the circuits in which the light is transmitted (70); it can therefore add nothing to the electromotive force. But in experiments of the former class, in which the acid is decomposed, if the action upon it be secondary (84), the fluid part of the circuit formed as usual of particles of water, is closed with a particle of acid, which gives up an atom of its oxygen to the last atom of hydrogen in contact with it; and the acid, thus in part disoxidized, supplies the electro-motive force, which in experiments of the latter class was communicated by the hydrogen.

90. These views are completely in unison with numerous facts derived from experiment. It is well known to chemists, that acids do not act upon metals unless they are previously oxidized; and that this oxidation is effected in the positive metals by the decomposition of the water with which it is necessary the acid should be diluted, and in the negative metals by the decomposition of a part of the acid to the action of which they are exposed. It is not then the superior affinity of the acids which are known to act upon the negative class of metals, that enables them to produce this effect; but it is the power which acids of this description possess, when rendered highly negative by being deprived of part of their oxygen (158, 174), to produce currents in a proper direction to effect the oxidation of the metal, under circumstances, when from the negative state of this metal, circuits terminating with hydrogen either cease to act, from the equality of the electrical forces, or produce currents in an opposite direction, in consequence of the metal being negative to the hydrogen. The metal, therefore, if the action upon it be a secondary result

(77 and note), is still oxidized by the decomposition of the water; but the hydrogen at the close of the circuit, instead of being set at liberty, is compensated for the oxygen relinquished to the metal by oxygen supplied from the acid; and the acid thus deprived of a part of its oxygen, being in this state more negative than hydrogen, produces an electro-motive force with the negative class of metals, similar in direction to that produced by hydrogen with the positive class. In confirmation of this statement, the oxides of silver and gold are readily dissolved by the sulphuric and muriatic acids, although these acids have no action upon the metals in a state of purity. The highly negative state of the oxide of azote, or whatever other name may be given to the nitric acid when deprived by these means of a part of its oxygen, is further confirmed by the superior electro-motive force produced when this acid forms part of the galvanic circuit (97). The nitric being one of those acids which act upon the negative class of metals.

91. It thus appears that the noble metals derive their distinctive character from the simple circumstance of their being negative to hydrogen. Metals which are positive to hydrogen produce a current in local circuits formed by water, from the hydrogen to the metal and from the metal to the oxygen, with decomposition of the water and the union of its oxygen with the metal. Metals which are negative to hydrogen, on the contrary, produce a current, or rather the disposition to do so, in an opposite direction, from the metal to the hydrogen; the tendency of which must be to increase the polar forces by which the water continues its present arrangement (87). By substituting a more negative body, such as the oxide of azote (90), for hydrogen at the close of the circuit, silver is brought within the class of oxidizable metals; but gold and platina, metals negative to silver, require still higher powers, which are probably given by substituting chlorine for oxygen at the commencement of the circuit.

92. It may be objected, that we have shown no sufficient ground for assuming the electro-motive force between the two negative bodies at a greater amount than the difference of the same forces arising from their action upon the positive body placed between them, in another part of the circuit. For instance, if the electro-motive force of hydrogen to oxygen be 6, and the electro-motive force of zinc to oxygen 5; and if this difference be produced by the difference of the electrical forces of zinc and hydrogen, why should the electro-motive force of the latter to the former be greater than the difference of the action of the same bodies upon oxygen (83)?

93. We admit the force of this objection; and, although

we have an explanation of the difficulty arising from it in view (108), we shall for the present take shelter under the authority of Fourcroy, by introducing an arrangement of his, in which he has made a similar assumption, under what appears to us to be nearly, if not precisely, similar circumstances. In treating of double elective attraction, he has given as an example, sulphate of potash and nitrate of lime. Sulphate of potash cannot be decomposed by either lime or nitric acid separately; but pour into a solution of the former a proper quantity of nitrate of lime, formed by the union of the two latter, and the nitric acid will quit the lime to unite with the potash. He has added the following table, in which it is assumed, that the attraction of sulphuric acid for potash is 8, the attraction of nitric acid for lime 4, the attraction of sulphuric acid for lime 6, and the attraction of nitric acid for potash 7. (Fourcroy's Chemistry, Vol. I. p. 65).

Nitre, or nitrate of potash.

		7		
Sulphate	Potash	divell	Nitric acid	Nitrate
of	8 quiescent	attract	attractions 4=12	of
Potash.	Sulphuric acid.	6	Lime.	Lime.
		13		

Sulphate of Lime.

94. In this table the numbers representing the relative forces of attraction, as in our own case, are evidently assumed to render them consistent with experiment; and in all probability the answer to the question.—Why is the difference of attraction between nitric and sulphuric acid for lime 2, while the difference of attraction between the same two acids for potash is only 1? would answer the question. Why is the electro-motive force of hydrogen to zinc 2, when the difference in their electrical states produces a difference of force towards the oxygen of only 1?

95. The table, as here constructed, although it shows a balance of force in favour of the new arrangement, gives us no conception of the manner in which the forces that leave this balance can be brought into simultaneous action. We must

recollect that the polar forces of the different elements are in contact in the original combinations; and that unless we can destroy these forces, by establishing others in more favourable positions for our purpose, the exchange must be commenced under circumstances of great disadvantage (66). The only way in which this can be accomplished, is to bring the elements into the form of a circuit, in the order in which their predominating affinities for each other will place them; when, supposing these affinities to depend upon the electrical state of the different elements, in accordance with our theory, and that the numbers representing their attraction for each other is also the representation of their electro-motive forces, a current will be established in the circuit in a direction which will reverse the polar forces, and bring them into positions in which the new arrangement known to take place must be the necessary consequence (63, 102). These circuits are represented in fig. 86; in which the direction of the electro-motive forces of the negative to the positive bodies with which they are in contact, is shown by the arrow heads, and the amount of force by the figures placed over them; the general direction of the current, arising from the balance of these forms, is indicated by the large exterior arrows.

(To be continued.)

LXVI. *On a new Theory of Stratification. By* MR. JOHN LEATHART, *Mine Agent.*[*]

The progress of geology, like that of many other sciences, has been somewhat retarded by the tendency of the human mind to theorize. It has too often occurred that scientific men have, without a sufficient groundwork of facts, built up a system, beautiful in itself, but without that foundation in truth which alone could constitute it truly valuable. Others, adopting these theories, before they have had sufficient opportunities of investigating facts, have been led by the prejudices thus imbibed, to take a one-sided view of whatever facts might be afterwards presented to their observation, and, instead of adapting their theories to nature, have endeavoured to distort nature so as to accord with their theories.

Having worked many years as a practical miner, before my attention was directed to scientific pursuits, my mind was stored with a variety of facts before my judgment had been warped by the adoption of any particular theory. And no sooner had I begun to study the science of geology, than I was struck with what appeared to me many inexplicable discrepancies between the appearances of nature and the theories of Geologists.

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[*] Communicated by William Norris, Land and Mine Surveyor, 4, Arundel Street, Strand, London.

The strata in Alston-moor and the neighbouring mining districts, to which my personal observations were almost entirely confined, are of the secondary formation, and consists of numerous alternations of limestone, argillaceous slate or plate, and siliceous rocks. According to one theory, I was to believe that the superposition of these alternating beds of rock, in the certain and determinate order in which they are found, has been the immediate result of successive depositions from water. According to another, I was to understand that these strata had been consolidated from loose sand or mud by an intense heat from the interior of the earth.

That the various materials composing these rocks might have been held in suspension by the waters of the ocean while in a state of violent agitation, and might afterwards have been deposited when the waters assumed a more quiescent state, I was ready to admit; but how, in a *mere mechanical deposition*, the materials of one stratum should first be deposited without admixture of particles of the second, and the second in like manner without admixture of the third, I was at a loss to understand; nor was I aware of any fact in the whole range of experimental philosophy which tended to elucidate that point.

Nor had I ever observed any appearances which seemed to indicate the operation of that heat, which according to other philosophers had effected the consolidation of these rocks.

The very definite divisions which exist between one stratum and another appeared to me quite different from what might have been expected to be formed in a continuous deposition from water. The occurrence of nodules of iron ore in clay and coal strata, and chalk in flints, presented another difficulty; for neither their internal structure, nor the promiscuous manner in which they are dispersed, could, I thought, be accounted for under the idea of a deposition from water.

For several years, I studied with a strong predisposition to reconcile, if possible, these and many other discrepancies between the actual phenomena of stratification and the observed laws of mechanical deposition from water. But multiplied observations seemed only to multiply difficulties, and I became convinced that the true theory of stratification had not yet been promulgated, nor could I at that time advance any more satisfactory hypothesis of my own.

I consequently gave up the study of stratification in despair of being able to find or construct a theory which would accord with my observations of facts: but, in the year 1830, being engaged in the study of galvanism, I found it remarked that many other substances besides metals would, when piled in alternate layers, develop electrical action.

It appeared to me, then, that as the strata consisted of alternating beds or layers of different materials piled on each other, they might form a species of galvanic battery, and a development of galvanic action might be produced in them, provided there were found to exist some medium uniting the extremities of these enormous piles; and mineral veins, descending from the surface to an immense depth, and being usually filled with some of the more perfect conductors of electricity, appeared to present a sufficient means of uniting the superior and inferior poles of these rocky piles, and thereby bringing their electric power into operation.

Pursuing the subject further in detail, I was led to conclude that each stratum, according to its position in the pile, must possess its own peculiar electric condition; and that, were any two contiguous strata in any part of the series to have their positions reversed, the electric condition of the whole series would be deranged. Also, that if the materials of any two contiguous strata in any part of the series should become mingled together, atom for atom so as to form a homogeneous mass, the atoms of each stratum retaining their own electrical condition, it appeared reasonable to conclude that the electro-motive action of the superior and inferior strata bounding the mixed mass would cause the atoms composing it to be drawn or attracted to their respective polar positions, and that it would thus be re-converted into distinct zones or strata.

Then, if by the electro-motive powers of the strata the atoms of any interposed mass of the mixed materials of similar strata could be selected and carried to their respective polar positions, it might reasonably be concluded that the same effect would be produced in the case of any other mixed mass of materials which may be interposed between surfaces in different electrical states; and generally, that if any mass of mixed materials, composed of particles in a different electrical condition, be interposed so as to constitute a conductor between two surfaces in different electric states, the particles of the mass will be separated and polarized, respectively, on the surface in an opposite electrical state to their own; the mixed mass being thus formed into distinct strata, more or less perfect, according to the energy of the electric action.

When the particles have accumulated on the polarizing surface to a certain thickness, the electro-motive action will be neutralized (as occurs with a galvanic battery), and the flow of particles will cease; then the stratum thus formed will, in its turn, become the electromotor in polarizing other particles of the mixture, which are in a different electrical state: this alternating operation being repeated until the whole of the mixed materials have been separated into distinct zones, thus forming an alternating series of strata.

Now, in the case of a quantity of sand or mud deposited at the bottom of the ocean we have the above conditions exemplified; for there is a mass of mixed materials between two surfaces (the ocean and the solid earth) in different electrical states, and if our suppositions be correct, such a mass would, by the electric action of those surfaces, be separated into distinct layers of the different materials composing it, that is, would be stratified. The degree of solidity and the texture of the strata might be expected to vary according to the thickness of the mass deposited, the intensity of the electrical action, &c.

Such is an outline of my views with regard to the stratification of the earth. These opinions were formed from pure reasoning, and dwelt in my mind for some years before I attempted to verify them by experiment. The following is a brief account of some experiments instituted with a view to test the accuracy of my conclusions.

Experiment 1. was performed with 28 pairs of cylindrical plates of copper and zinc. The copper plates having a surface of about nine square inches, the zinc about six square inches. These plates were placed in jars containing a fluid composed of $\frac{59}{60}$ of water, and $\frac{1}{60}$ of muriatic acid. The materials operated on were limestone and sandstone, reduced to a fine powder, thoroughly mixed and made into a paste with water. This mixture was put into a glass tube, half an inch in diameter, and an inch and a half in length. Metallic discs, fitting the bore of the tube, having been soldered to the ends of the connecting wires of the battery, were inserted so as to constitute the materials in the tube a part of the connection between the poles of the battery. The experiment was continued until the battery became null, and the result was a decided appearance of stratification, although the separation of the ingredients was very imperfect. The materials operated upon were firmly united, especially towards the centre, and adhered to the glass so strongly that it was impossible to separate them without breaking it.

Considering that the operation, as performed by nature, must be more slow and gradual than that in the above experiment, I resolved in future to use common spring water instead of the acidulated water; and in all my experiments performed since, I have used water only as an exciting fluid.

Experiment 2. The battery used was the same as before, excepting the fluid. The materials operated upon were a mixture of pounded limestone and argillaceous shale. The action was continued eight days, at the end of which time I extracted the mass from the tube. The separation was more perfect than in the first experiment, but there was less consolidation; the materials easily crumbling into powder. This I attributed to the limited time during which the action was continued.

The limestone was collected on the negative or zinc end of the battery, and the shale on the positive.

Experiment 3. The materials operated on were limestone, sandstone, and argillaceous shale, pounded and mixed as before.

In ten days a very distinct though not an entire separation was effected, the limestone occupying the negative end, the shale the positive, and the sandstone being collected in the centre. The induration of the limestone and shale was but slight. The sandstone was much more compact and solid.

These experiments were repeated a number of times, with very little variation in the results: but I found that the operation was impeded for want of moisture, the water used in mixing the materials being decomposed in a few days, after which the progress of the experiment was scarcely perceptible.

To obviate this I had holes drilled through the tubes, through which I was enabled to supply the mixture with water; after which the results of my experiments, both as regards the separation and induration of the materials, were more satisfactory than before.

For upwards of six months, during the latter part of the year 1836, I had two and sometimes three batteries, of from twenty to thirty pairs of plates each, operating on different rocky mixtures.

The success of the experiments varied, but the results generally were such as stated above; tending most decidedly to confirm me in the opinions I had formed with regard to the stratification of the earth.

In the course of my investigations, a variety of curious phenomena presented themselves, illustrative of important facts in the science of geology.

It may be proper to state that the position in which the tubes are placed, has no influence on the results, for I have placed them in every position, from the horizontal to the vertical, without being able to perceive that the experiment was at all affected by the change.

My views are not confined to the stratification of the secondary rocks, but extend to the other formations and to the undulations of the earth's surface. These I cannot enter upon in this communication, but will be happy at some future time to explain more fully the details of a system which will give a more simple explanation of many complicated geological phenomena, and present a more harmonious union and identity of cause in the operations of nature in the animal, vegetable, and mineral kingdoms, than has hitherto been attempted.*

JOHN LEATHART.

Alston, Cumberland.

LXVII. *On Mr. Robert Were Fox's Theory of the Electrical Origin of Metalliferous Veins. By* M. BECQUEREL. *(Traité Expèrimental de l'Electricité et du Magnétisme. Vol.* V. *p.* 167, 174*).*

Translated by Mr. Thomas Henwood.†

Mr. Fox remarks that, if we consider the electrical relations of the different metallic ores, in a geological point of view, we observe that almost all those which are generally associated in the same veins, agree in the particular, that their reciprocal voltaic action is generally very slight. Hence he infers that if it were otherwise, the appearances of decomposition, in the same locality, would be much more general and decided than they are found to be. He remarks also that when copper pyrites and vitreous copper ore form a voltaic combination with pure or spring water the electro-magnetic effects are considerable. We would merely observe that Mr. Fox appears to be ignorant that the electro-chemical effects produced in the contact of two solid bodies and a liquid, depend solely on the chemical relations of their constituent parts and must frequently vary.

Let us now come to the researches of the same gentleman on the electro-magnetic properties of the metalliferous veins of Cornwall, in which he has been engaged for the last six years.

* We shall at all times be very happy to place Mr. Leathart's experimental labours and theoretical views before the readers of these Annals. EDIT.

† Communicated by W. J. Henwood, Esq.

The apparatus which he has used in examining these properties, is composed of small plates of copper fixed with iron[*] nails, or pressed strongly by means of wooden stays, on the portions of the veins submitted to experiment, and put in communication with the two extremities of the wire of a multiplier, with short wire and with needles of which the directive forces were not compensated.

Mr. Fox says, he observed with this apparatus the following effects: the intensity of the current varies according to the localities; sometimes the deviation of the magnetized needle is slight, sometimes it is very considerable; in general, it is the greatest when the vein contains a greater quantity of copper, and perhaps even from the depth of the stations. He adds that there is no, or scarcely any, action perceptible where there is but little metallic substance. Where there is a distance of but a few fathoms between the plates, in a horizontal direction, and also a large quantity of copper not interrupted by non-conducting substances, there is no action; but if there be, by chance, quartz or clay in the vein, the action is generally very decided.

When the two plates are placed at different depths in the same vein, or in different ones, the electrical action is in general very evident; the electric currents are sometimes in one direction, sometimes in another. In comparing parallel veins, he thinks he has remarked that positive electricity takes a direction from north to south, although in some cases he has observed the contrary. In veins dipping towards the north, the east is generally positive and the west negative. He has found in comparing the relative states of veins at different depths, that the lower stations appear negative in relation to those above them. He has, however, found some exceptions, particularly when a cross vein of quartz or clay intervenes between the plates. There is, therefore, no regular order in the direction of the currents.

If, indeed, there were a progressive increase of negative electricity as we descend in the mines, this phenomenon would agree with the progressive elevation of temperature. The electrical effects are not, according to his account, influenced either by the presence of the workmen, their lights, or by the explosion of gunpowder, &c.

All substances that form part of metalliferous veins, are far from possessing the conducting powers necessary to allow the passage of currents transmitted by the metallic portions. He classes (as we have seen) among conductors the ores of copper nickel, copper pyrites, vitreous copper ore, iron pyrites, arsenical pyrites, galena, arsenical cobalt, the crystallized peroxide of manganese, and fahlerz: among the non-conductors, he places the sulphurets of silver, of mercury, of antimony, of bismuth, of arsenic (realgar), of manganese, and of zinc, the combinations of the metals with oxygen and the acids.

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[*] Nails were not very often used, but those that were, were always of *copper*. W. J. H.

Mr. Fox assures us he has discovered that the Cornish rocks, the beds of clay-slate appear to possess the property of conducting electricity in a slight degree, but only in the direction of their cleavage. This effect can be attributable only to the water interposed.

With regard to the electric properties of metalliferous veins, he observes, that substances which conduct electricity, have generally, at least in this County (Cornwall), non-conducting substances interposed in the veins, between the conductors and the surface. He cites the veins of tin, which are generally intersected by those of copper. When they do not coincide in their horizontal directions, the conducting veins traverse those that are not so.

Mr. Henwood, who has been more recently engaged than Mr. Fox in examining the electric currents in the mines of Cornwall, has stated, that the veins on which they are worked, traverse both the granite and the micaceous (slate) rocks; that they are principally composed of quartz and other earthy minerals, mixed in many places with copper pyrites, iron pyrites, vitreous copper ore, oxide of tin, blende, galena, with mixtures in small quantities of native copper, protoxide and carbonate of the same metal, and some of the salts of lead. At the greatest depth the temperature of the micaceous (slate) rocks is two or three degrees higher than that of the granite at the same level.

In many very deep mines, the water contains salts in various quantities; among others the chlorides of calcium, of sodium, of magnesium, &c.

Mr. Henwood has employed the mode of experimenting already described. The metallic plates were placed at distances varying from a few feet to many hundred feet, at the same depth, and at different depths.

The results have been the same whatever the direction of the veins might be. In those which only produced tin, and in many which afford copper, no traces of a current were perceived, except in some cases where the intermediate space was filled with rich copper ore.* The presence of electricity was more decided when the vein contained copper pyrites, vitreous or black copper ore, galena, or blende ; it was not detected when there were no metallic substances exposed. Some veins contained copper pyrites, grey copper ore, and galena; others carbonate and phosphate of lead, and grey copper, which exhibited no traces of electricity.

It appears that Messrs. Fox and Henwood have not remarked the relations between the directions of the veins, and those of the currents.*

In experiments where they have connected portions of metalliferous veins at different levels, the currents have in thirteen cases been upward, and in thirty-five instances downward.

In thirty-six observations the currents have been towards the granite, and in twenty-one others it has taken an opposite direction.

We do not know whether Messrs Fox and Henwood have sufficiently guarded against all the sources of error which present themselves when we seek to prove the existence of electro-chemical currents by means of a multiplier. The results which they have announced are so important, in relation to the electro-chemical reactions which operate in veins, that we must examine them.

To enable the reader to judge and to appreciate the accuracy of Mr. Fox's mode, we will remind him that when two plates of platina, connected with the extremities of the wire of a multiplier, are immersed in distilled water, a current is immediately produced by the difference of the actions which the water exercises on foreign bodies adhering to the surfaces of the plates. This effect almost invariably takes place when the precaution is not taken to remove the foreign substances which adhere to the surface of the platina when we remove the plates out of the water. If, instead of platina, we use copper, the current is still more apparent, as the surfaces are not the same, and the water does not equally affect them.

Now, Mr. Fox, in his experiments, has used plates of copper, which he has fixed on the metalliferous veins with iron nails, or pressed them by means of wooden props. These two plates were connected with the multiplier by means of a copper wire.

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* Some misprint or misapprehension appears to exist in this passage, as the table on page 171 clearly gives the directions of the veins. The purport of the paragraph should be "in all those producing tin ore alone, and in many which afford copper, and in most cases where there was a continuous mass of copper ore between the points examined, no electricity was detected. In some instances, however, where all the intervening space consisted of rich copper ore, most energetic action was detected." *Annals of Electricity*, Vol. I. p. 127. W. J. H.

Might it not happen that the water which adheres more or less to the sides of the galleries of the mine, and which do not every where contain the same salts, have the same effect on the copper as in the experiment just mentioned? It were to be desired that Mr. Fox should make his experiments on parts of conductors of electricity which are perfectly dry; the objection which we have just made would then fall to the ground.*

It is true Mr. Fox, who has experimented with plates of copper and of zinc alternately, has observed in both cases the direction of the current was the same; the result is so far favourable to his opinion, but it is not sufficient to demonstrate the fact completely.

In order to show in what manner electric currents have acted and are still acting on metalliferous deposits, some persons have recently advanced that the veins have been filled by the action of electric currents; but it is only necessary to study the manner of filling these mineral deposits to reject such a theory.

We know that veins are fissures which exist in most of the rocks composing the crust of our globe, and which are filled with metallic or stony substances. Opinions are divided as to the manner in which this was effected; some say by igneous, others by aqueous, action. Werner was of the latter opinion. According to this celebrated Geologist, the substance of which mountains are composed was moist and yielding, afterwards as it settled and dried, fissures were formed which were filled from above by matters which had been dissolved; but as there are some veins which appear to have been filled from below, we must admit their having been filled by sublimation. Hutton, who is the great advocate of the igneous theory, supposed the internal heat of the earth so great as to melt and vaporize the metals and earths, which from their expansive force have produced fissures in the solid crust of the globe, through which they have escaped, and in solidifying have thus given birth to the crystalline rocks; thus he accounts for the formation of large trap dykes which traverse the strata of all ages.

Taking this view, veins have been opened by elevatory forces and filled from beneath by sublimation, and from above by substances which have, by various causes, been destroyed and removed from the surface, and which in the veins have remained unaffected.

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* It is very seldom that perfectly dry surfaces can be found. They were so (except from the dampness of the atmosphere) when the existence of a current was detected in the (N and S) vein of *Levant Mine.* W. J. H.

All the facts hitherto observed induce us to believe that we cannot admit one of these hypotheses to the exclusion of the other, as each of the supposed causes may have operated, according to circumstances, in the filling of veins of various descriptions. Geologists consider it certain, that the veins which contain the debris of rocks, constituting the upper strata of the country which they traverse, and organic remains, have been filled from above; but it is very different with veins which are connected with the contiguous rocks by gradual mineral transitions. In this case we are compelled to admit that the formation of the rock, that of the vein, and its filling up, have been almost contemporaneous. On the other hand when we see crystallized masses of different substances in the middle of rocks equally crystalline, surrounded by them on all sides, in such a manner that it is impossible to say they have ever been introduced into the cavities which they fill, either from above or beneath, we are compelled to consider the vein as in some cases a fissure in the middle of a crystalline rock, filled by substances afterwards introduced in a state of solution, and varying more or less from the nature of the rock itself, as the solution was more or less saturated. There is another mode of filling: and it is that which has produced the veins which contain metallic sulphurets deposited in crystalline groups, standing out on all parts of the vein, and all readily decomposed substances in aqueous solutions, such as the sulphurets, the metallic arseniurets, which will not sustain an elevated temperature without decomposition, unless the action be under the influence of considerable pressure. It therefore appears to be almost certain that all veins have not been produced by one general cause alone, and that many influences have sometimes concurred in their formation.

It follows, from the rapid sketch which we have just presented of the state of our knowledge of the constitution of veins, that it is impossible to admit that the fissures, which have at different times opened in the rocks, have been filled by substances conveyed thither by terrestrial electric currents; as these currents exert no chemical action except where there are solid conductors and liquids capable of reacting on each other; now the rocks are not conductors of electricity, and solid bodies can only be metallic combinations which did not exist at the period when these fissures were produced, we must therefore admit that other causes than electric currents have filled these openings.* The filling up once effected, whether partially or entirely, and the water coming from the contiguous rocks, electric currents would then interpose and give birth to new combinations.

LXVIII. *On the primitive colours of light.* *By* PAUL COOPER, ESQ.†

In a series of papers on the composition of white light, published in the Records of General Science, I endeavoured to show, and I think successfully, that red, green, and violet, are the only primitive colours; that red and green form yellow; that violet and green form blue; and that red and violet form crimson. "No shade of colour can be formed of more than two of the primitive colours; for the moment the third is added, its complementary colours form white light with it, and they disappear together, leaving the colour or colours which happen to be in excess, diluted with the white light thus formed. It is upon this principle that when blue and yellow are superposed, green makes its appearance: there being a surface of this colour combined with violet to form the blue, and another surface combined with red to form the yellow; a mixture of the two colours must necessarily leave a surface of green in excess." (Vol. II. p. 31.)

In the course of numerous experiments made for the purpose of elucidating this subject, I discovered that yellow glass of a deep shade, inclined to orange, is nearly opaque to violet light; that the light admitted by it through a narrow aperture, analysed by viewing it through a prism, consists of only two colours, green and red; and that when the spectrum in any stage of its development is viewed through it, the violet disappears and the blue is converted to green. This experiment may be made by viewing an object at some distance through a prism, so as to produce a considerable breadth of blue and violet light, and by occasionally interposing one of these glasses either before or behind the prism. The sudden disappearance of the violet light and the conversion of the blue to green upon the interposition of the yellow glass produces a striking and beautiful effect.*

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* In the Annual address to the Geological Society, delivered by the President, the Rev. W. Whewell, M.A., F.R.S., on the 16th of February, 1838, when speaking of Mr. Fox's theories, he says "the discovery of the causes of the forming and filling of metallic veins, one of the earliest subjects of geological speculation, will remain probably as a problem for its later stages, when our insight into the laws of slow chemical changes is far clearer than it is at the present day." The electrical theory seems in not much higher favour with Mr. Whewell than with M. Becquerel. W. J. H.

† Communicated by the Author.

These experiments were brought to my recollection by reading the article on the Analysis of Solar Light in the last number of the Annals. Chemical action is known to be produced by the most refrangible rays only; and, according to the theory above stated, these rays are wholly absent in yellow light. Hence the light admitted by the several *yellow* metallic solutions enumerated at the close of this article is separated from the rays which produce chemical action. The exceptions there stated probably arise from the violet rays not being wholly excluded; in which case, the yellow, if received upon a white ground, will be pale, in consequence of being diluted with white light. On the other hand, the light admitted by a solution of sulphate of copper and ammonia is wholly of the most refrangible kind, and though it possesses little brilliance as light, its chemical effect is considerable.

It has not, I believe, been decided whether any chemical ray exist of a higher refrangibility than violet light; but it is probable if there be such a ray, that a medium opaque to the latter will also refuse transmission to the former. With regard to heat, I think there can be little doubt that there are rays which produce it, of a lower degree of refrangibility than those which are visible to us; and though it does not follow as a necessary consequence, it is probable, that a medium which refuses transmission to the least refrangible coloured rays, will also refuse it to radiant heat. But there may be, and it appears from the experiments of Dr. Draper there are, media transparent to light of all degrees of refrangibility, and yet nearly opaque to radiant heat; and others, which, though they transmit light of the lower degrees of refrangibility, are also opaque to heat.

There is some confusion in the account given of these experiments, from a want of the necessary distinction between the different sides of the spectrum; undoubtedly "there are solutions, which are transparent as respects the sun's light," (viz. the most refrangible part of it) "yet opaque to his calorific ray, and others which are transparent both to his light and heat," (the least refrangible rays) "but opaque to the chemical ray."

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* This glass may be met with in most glaziers' shops. The piece with which I have made many of my experiments is coloured on one side only, the colouring matter having penetrated only a thin film of the glass; and yet it appears to be quite opaque to violet light, with the exception, in a trifling degree, of the direct light of the sun. I hope some of the readers of the Annals will repeat these simple experiments; and, if they are advocates for the primitive character of blue light, that they will endeavour to give us an explanation of them upon this principle. Upon the principle of its being a compound of violet and green there is no difficulty.

It would be unfair to draw a positive conclusion from a bare outline of experiments like the present; but these experiments, as they are given in the paper referred to, do not appear to me to decide the question of the separate existence of chemical rays in solar light, there being no evidence of chemical action in the absence of the violet ray, or of the want of it when it is present. With regard to the separate existence of calorific rays, I believe, previously to the appearance of these experiments, there was very little doubt on the subject.

LXIX. *On Electrical Theories, and Voltaic Experiments. By* J. Harper, Esq. *In a letter to the Editor.*[*]

Oxford, March 29, 1838.

My dear Sir,

I have read with much pleasure the proceedings of the Electrical Society, and much wish I could be a resident member, that I might have an opportunity to attend the meetings.

I have with you considered the Franklinian theory the most simple and best adapted to explain the usual experiments; but I am at the same time not so wedded to it as to be unwilling to listen to or read whatever can be said or written on any other, and have entertained doubts whether we are not still ignorant of the true one. These have arisen from the charging the Leyden jar; for if glass be really a non-conductor, how can the outside coating be affected by the inside lining? And again, how can pith balls be put in motion by a chain from the prime conductor of the machine communicating with the bottom of an inverted tumbler glass enclosing the balls?

I should feel greatly obliged if you could propose for discussion at the Society's meeting, why, when a spotted jar is only spotted on the *outside* and lined with tin foil in the usual way with unspotted jars, the sparks appear only when the jar is discharged; whereas when spotted both inside and out, the sparks appear while it is charging as well as on the discharge?

I have been trying an experiment with the voltaic battery by substituting a metal cylinder for a membrane; the idea occurred to me that as metal was a good conductor, why might it not be used to supersede the bladder, &c., which is objectionable, and having a thin brass tube about 12½ inches in diameter, I fitted a cork to it to make it water tight at bottom, and enclosed in it a zinc cylinder, separated by pieces of wood, to prevent contact, and placed the whole in a porcelain jar lined with a cylinder of copper, likewise kept from contact with the brass tube. Between the brass tube and copper, the space was filled with a solution of sulphate of copper, and within the tube with dilute sulphuric acid. A copper wire was soldered to both zinc and copper cylinders, which wires, when severally immersed in cups of mercury in which also were the two ends of a helix of copper wire enclosing a piece of iron wire, an arrangement of course the same as when the usual batteries are employed; the magnetic effect on the iron wire was decisive so as to show that it acted, though feeble, and the action on the sulphate of copper was decisive by the metal being reduced and forming a film of copper on the copper cylinder, so that my expectation was realized.

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[*] Read before the London Electrical Society, Saturday, May 5th, 1838.

I remain,
Dear sir,
Yours truly,
J. HARPER.

LXX. *New Observations on the measure of the temperature of the organic tissues of men and animals' bodies, by means of thermo-electric effects, by* MM. BECQUEREL and BRISCHET.*

The memoir we now present to the academy is a succinct exposition of the continuation of experiments we undertook at Paris, and in our voyages to the Alps and Italy, for determining, in a more rigorous manner than has yet been done, the temperature of the tissues in general, and of the interior organs of man and animals, by the assistance of thermo-electric effects.

The use that we have made of the mixed metallic needles, less than a millimetre in diameter, for determining the temperature of the interior parts of organized bodies, require some delicate precautions with which we have already made some persons acquainted, and without which it is not possible to obtain results, on the accuracy of which we might depend;

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* From the Compte Rendu des Séances de l'Académie des Sciences. April 9th, 1838. Translated by Mr. J. H. Lang.

we are now going to complete what we have already said on this subject.

When one of the extremities of a metallic bar is plunged in a source of heat, which is not capable of reacting chemically on its constituent parts, this bar becomes heated, by degrees, to a greater or less distance from the immerged part, according to the nature of the metal, the dimensions of the bar, the temperature of the source, and that of the surrounding air.

Hence the different sections of the bar, above, and to a certain distance from, the source of heat, assume different temperatures superior to that of the surrounding atmosphere; but as soon as each of them attains the temperature it is to preserve, that is to say its state of equilibrium; experiment proves that for distances from the source which increase in arithmetical progression, the excesses of temperature decrease in geometrical progression, whenever the excess of the temperature of the bar over that of the surrounding medium does not exceed 20 or 30 degrees. On the other hand, the propagation of the heat varying according to the dimensions of the bar, the loss of heat being proportional to the area of the exterior surfaces, and the quantity of heat which traverses being also proportional to the area of the section, it follows, that the decrease of temperature will be as much greater as the circumference is less. Experiment effectually proves that in two bars of the same metal, not having the same transverse section, the distances of the focus from the points in which the temperature is the same, are to one another as the square roots of their thicknesses, or as the square roots of their radii if the bars be cylinders. It follows from these different observations that the smaller the diameters of the cylinders or metallic needles, the less the source of heat would become cooled when its temperature would be capable of varying by the presence of these needles: hence the necessity of operating with needles whose diameters are less than a millimetre.

It also follows, from the preceding observations, that when we seek to determine the temperature of the interior parts of a man who is about 37 degrees, we must place him in a medium whose temperature is at least 18 or 20 degrees. If this condition do not yet suffice, we must find by previous experiments, the effects due to the cooling produced in the muscles by the presence of the needles. This is a point on which, perhaps, we have not been sufficiently determined in our preceding memoirs.

The process for finding the interior temperature of the human body consists, as is known, in making use of two needles each composed of two others, one of copper and the other of steel, soldered at one of their ends. One of them is placed in a medium whose temperature remains constant during the time of the experiment, whilst the other is introduced into the part the temperature of which we wish to measure. These two needles are connected on one side, by their steel end, with a steel wire of the same nature, and on the other, by their copper end, with the extremities of the wire of an excellent thermo-electric multiplier.

When the two soldered needles have the same temperation there is no deviation of the magnetic ones; but for the least difference in the two temperatures, be it only 0·1 of a degree, there is a deviation whose direction and extent serve to estimate correctly this difference, and consequently the temperature of one of the media, when that of the other which is constant is known.

The constant source that we usually employ is furnished either by the apparatus of M. Sorel, which has already been described, or by the mouth of a person accustomed to this kind of experimenting. Sprel's apparatus preserves for some hours, a temperature, only varying a few tenths of a degree; but the mass of water which gives it is so considerable, that the solder immersed therein is immediately put in equilibrium of temperature with it, notwithstanding the losses experienced by the parts of the needle placed on the outside, which are quickly repaired. In this case, the temperature shown by the solder is that of the medium in which it is found. It is not the same with the temperature shown by the second solder, which is found in a muscle a small distance from the skin, which muscle, by reason of the tissues of which it is composed, from their small extent and bad conductibility, ought not to be considered as an equal source of heat to the other; we also find when operating in a medium, whose temperature is below 18 or 20 degrees, a difference in favour of the apparatus, even when the temperature of the latter is the same as that of the muscle.

By using the mouth as the source of constant heat, we have not to fear so much the differences that we have just shown, because the two sources have an analogy among themselves, with regard to their constitution.

We have entered into some details on the precautions to be taken, when we endeavour to measure the interior temperature of organized bodies, in order to enable those persons wishing to make use of our procedures to avoid the indicated causes of error.

We shall now mention the experiments we have made for showing how far the mouth may replace the apparatus for constant temperature.

Each of the solders was placed in the mouth of a young man 22 years of age, between the palate and the tongue, which exercised a slight pressure on the metallic wire, so as to avoid the variations resulting from the passage of the air breathed. The magnetic needle deviated 1½ degrees in favour of one of the two mouths. The solders having changed mouths, the deviation was 2 degrees in another direction, instead of 1½ degree. The difference of half a degree, corresponding to $\frac{1}{10}$ of a degree of temperature, proceeded very probably from the solders not having been placed alike in the two experiments; the effects did not vary for a quarter of an hour.

Hence we see that with certain precautions we can make use of the mouth as a source of constant temperature, when we are accustomed by previous attempts to keep the solder always in the same position, and to breathe through the nose, so as not to introduce cold air into the mouth.

One of the solders having been placed in Sorel's apparatus marking 36 degrees, the other in the mouth of a young man, the deviation of the magnetic needle was two degrees in favour of the mouth, which indicated a temperature of 36° 40′ instead of 36° 50′, shown by the thermometer; a very slight difference owing to unseen causes.

The one solder was left in the mouth as it was, and the other was placed in the biceps muscle of the second young man, the temperature of the air being 14 degrees, consequently, below what is necessary for the success of the experiments, we had a deviation of 4 degrees in favour of the mouth; hence, the temperature of the biceps given by the needle was only 36° 20′ instead of 36° 60′, which is the mean temperature we have found in our preceding memoirs.

The solder which was in the mouth, was taken out to be placed in Sorel's apparatus, which showed 38° 50′ by the centigrade thermometer; the deviation of the magnetic needle was 10° in favour of the apparatus; hence, the mouth possessed a temperature of 36° 50′, as we have previously found it. Thus the mouth may be used with advantage as a source of constant temperature.

We have naturally been led to make some experiments on the influence of the variations of the surrounding temperature on that of the human muscles. This question which has occupied philosophers and physiologists for some years past is not yet completely resolved, wherefore the results that we have obtained will not be without interest for the science.

It is certain that man, as well as warm blooded animals, can live in an atmosphere which differs nearly 80 degrees in temperature from their own, since the inhabitants of the polar regions, covered it is true with clothes, are one part of the year exposed to a temperature at which mercury freezes. Hence, man as well as warm blooded animals possess in themselves the faculty of increasing in a given time the heat that they develop. As to the faculty which they have of resisting high temperatures, without any sensible disorder in the animal economy resulting, we shall refer to the experiments of Banks, Blagden, and Fordyce, who have remained exposed for some moments to a temperature of 125 degrees, without finding any sensible change in their own, estimated probably from that of the mouth.

On the other hand, Berger and De la Roche, having been exposed to a temperature of 49 degrees found theirs increased 4 degrees; and De la Roche, having remained alone in a hot-house at 90 degrees, for sixteen minutes, has proved that his was only increased 5 degrees.

Captain Parry relates, that in the polar regions, where the temperature is lower than that at which mercury freezes, that of man is not sensibly modified. This last observation is contradicted by Mr. John Davy and some others, who have found that the temperature of man increases from the poles to the equator.

Without wishing to enter into an examination of the contradictory results we have just mentioned, we shall confine ourselves to mentioning the experiments we have made on the same subject.

We introduced to the biceps muscle of the right arm of two young men, each of the solders of two perfectly similar needles, the temperature of the surrounding air was 16 degrees, the magnetic needle showed no appreciable deviation; hence, the two muscles had exactly the same temperature. One of the arms under experiment was immersed as far as the elbow, incessantly for a quarter of an hour into water, at 10, 8, 6 degrees, then at 0; the experiment lasted about an hour; the deviation of the magnetic needle was only two degrees in favour of unimmersed muscle, which indicates a diminution of temperature in the other of about the fifth of a degree.

The same arm having been afterwards plunged in water at 42 degrees for fifteen minutes, the temperature of the immersed muscle was only increased the fifth part of a degree.

These experiments having been repeated at different times, we have always found but very feeble differences in the temperature of the muscles.

These results have been confirmed by the experiments we have made at the mineral water baths at Lovech, in Valais, two years ago, and recently at Paris, with the assistance of M. Seguin, external pupil of the Hotel Dieu, at Paris, who much wished to assist in our researches with a zeal worthy of praise. We were not contented with putting the arms in the water at an elevated temperature, but immersed the whole body therein. The waters of Lovech were 49° centigrades.

The temperature of Sorel's apparatus indicated 35° 50′: one of the solders was placed in it, while the other was introduced into the biceps muscle of M. Seguin, the deviation of the magnetic needle was 12 degrees in favour of the muscle, which indicated a temperature of 36° 70′. M. Seguin having been placed in the bath at 49 degrees, remained there twenty minutes; the deviation of the magnetic needle varied from 12 to 13 or 14 degrees according as it was more or less distant from the water. Hence the temperature of the muscles increased from one to two fifths of a degree. On coming out of the bath the deviation of the magnetic needle returned to 12 degrees as it was before. M. Seguin's pulse made 112 pulsations per minute in the bath.

We obtained the same result on a vigorously constituted young Tyrolean carpenter. We were unwilling to repeat the experiments at a higher temperature, for fear of injuring the health of persons volunteering to assist in our researches. But we have recommenced them at Paris at a temperature a little lower than 49 degrees, with the assistance of M. Seguin and M. Castille, also external pupil of the Hotel Dieu. One of the solders was placed in M. Costille's mouth, the temperature of which, measured by the thermometer, was 37° 50′, the other in the biceps muscle of M. Seguin's right arm, the deviation of the magnetic needle was 2 degrees in favour of the mouth, which indicated a temperature of 37° 10′ for the muscle. M. Seguin was placed in a bath at 42° 50′ and remained there twenty minutes; the temperature of the muscle was not changed, as the deviation of the magnetic needle remained the same.

This experiment repeated on M. Castille gave the same result. We see by the facts just mentioned, that when the human body is in contact with water, whose temperature varies from 0 to 49 degrees during a space of twenty minutes, that of the muscles experiencing only feeble variations: perhaps it would be the same if the contact were prolonged for some time, as the experiments of Mr. John Davy and other philosophers lead us to believe; but it is impossible to verify this assertion, since very serious disorders in the general economy would result from it: a bath of 49 degrees already strongly reddening the skin and terminating the blood to the head.

We may also conclude some observed facts, that the results obtained by M. De la Roche, who was placed in a hothouse at a temperature of 49 degrees, are due in a great measure to the phenomena of respiration, which modify the temperature of the mouth.

We shall also relate one experiment made at Lovech, and which has not been repeated on account of the difficulties it presents. This time it was a dog on which we experimented; his muscles indicated a temperature of 38° 50′; plunged in a bath at 49 degrees, the needle not touching the water, the temperature of the extensor muscle increased successively from half a degree to 1, 1½, and 2 degrees, and that in the space of five minutes. The dog became so furious that we were obliged to withdraw it from the water; after a short time the temperature of its muscle returned to what it was at first.

The solder was introduced into his chest, and we obtained equally an increase of temperature of several degrees some moments after the immersion in the bath: this increase took place chiefly when the animal was violently agitated. We are ignorant of what influence the exasperated state of the animal had on the effects that we have observed. We shall also mention a curious result, which has no relation to the preceding ones, but which will interest physiologists.

One of the solders was placed in the biceps of a young man, the other in the great supinator muscle of the left arm of a man 45 years old. The magnetic needle underwent no sensible deviation. The vein was opened, but we observed no change of temperature during, and after the loss of the blood. The solder was placed as near as possible to the vein. We may draw what conclusion we please from this fact; but the only one which appears natural to us, is that *a priori*, we ought to think that it would be thus, because the blood, whose escape was permitted by the opening of the vein, returned to the heart, and having already circulated through the capillary vessels, has become foreign to the composition of the tissues in returning to the central organ of the circulation by the branches and veinous trunks. Hence it could only produce a decrease of temperature in the animal body by its abundant flowing out, and producing a weakness of the subject. We therefore thought it right to make the experiment in another way; on which account we took a middling sized dog, which had eaten a few hours before the experiment, and placed one of the solders in the muscles of the fore part of the thigh, while the solder of another needle was in the mouth of an experimenter, a bandage having first been thrown round the femeral artery, immediately below the outlet of the abdomen. The suspension of the blood's course in this vessel, caused no change in the temperature of the limb, and by several repetitions we exercised or suspended the compression on the arterial trunk, without being able to observe the least motion in the needle of the multiplier.

Would it be necessary, in conclusion, that the modifications in the temperature of the tissues, depend much less on the sanguinary circulation than on the nervous influx, or even that the results of this last experiment prove that, in only tying the femeral artery, we have not stopped the whole of the blood in the vessels of the thigh, the fessiery and ischiatic arteries being able to make up for the femeral one.

In order to have a positive solution of this physiological difficulty, we have embraced the primitive iliac artery with a double silk cord; then placing one finger on the vessel at the point answering to the pression of the bandage, we could at pleasure hinder or permit the circulation of the arterial blood in the limb. The needle was then engaged in the thickness of the fleshy parts of the thigh, and at the end of eighteen minutes we had perceived the temperature lower about half a degree. Afterwards, permitting the blood to traverse the femeral arterial vessels, the temperature was soon re-established in its normal state. This experiment repeated several times gave us the same result; although the effect observed be very feeble, it shows, nevertheless, that the arterial blood exercises a direct influence over the temperature of the tissues; it is not, however, to the blood which circulates in the trunks and arterial branches that we must attribute this influence; but that which traverses the plexus capillaries. In fact fifteen or twenty minutes usually elapse between the suspension of the blood's course in the limb, and the diminution of the temperature. However, the re-establishment of the temperature in its normal degree, when the blood is permitted to traverse the arteries, was always more rapid than the diminution of temperature when the principal vasculary trunk was compressed.

We have here stated what relates to the influence of the arterial circulation over the temperature of the animal tissues; in another memoir we shall mention what experiment has taught us of the nervous influence, with regard to this same temperature of the tissues.

The facts that we have just related in this memoir, show anew what we may deduce from the thermo-electric effects, to estimate the temperature of the interior parts of man and animals; taking as a constant temperature either that of Sorel's apparatus or that of the mouth of a person accustomed to this sort of experimentalizing.

LXXI. *Repetition of Mr. Fox's experiments on the Stratification of Clay by Voltaic Electricity.*

A repetition of Mr. Fox's experiments was begun on Tuesday the first of the present month, as promised in the last number of the Annals, p. 395. The pot which contains the materials of the voltaic arrangement is of the common white crockery. Its figure is oval, being 6 inches long, 4 broad, and 2 deep. The clay on which the experiment is making was well kneaded with rain water, and formed into a partition across the shorter diameter of the pot; dividing it into two compartments of nearly equal capacity. Whilst forming and placing the partition, particular care was observed to prevent mechanical stratification in the clay; a circumstance which ought to be strictly attended to in all experiments of this kind; because an inattention in this particular might lead to very great error, by the stratification caused by *kneading* the clay, and forming it into the partition, being mistaken for an electric effect.

In every particular in the arrangement, I have attended to the directions given by Mr. Fox, at page 54, of the present volume. There are, however, some other particulars in the arrangement which will be necessary to mention. Having failed in several trials, to unite the conducting wire with the copper ore by means of solder, I was at some loss to know how I should manage to keep up the connexion without immersing the copper wire in the solution: a circumstance which I thought might possibly appear to be of some consequence in the value of the experiment whatever might be the result of it: though Mr Fox has not noticed it in any of his descriptions of his apparatus. Having, however, a printed figure of that gentleman's apparatus sent to me, which obviously shows that the connexion is made by passing the copper wire round the ore, and twisting it tight to maintain its hold, I have adopted the same plan. Indeed, as the experiment is intended as an exact copy of the original one, no variation in the arrangement ought to be permitted. The copper wire, therefore, forms one of the metals of the voltaic combination; the copper ore another on the same side; and a plate of zinc the other; which is attached to the wire by soft solder. The solution of sulphuric acid is very weak.

Prior to the final arrangement of the materials, I ascertained that the copper wire and the zinc produced a much stronger current than the copper ore and the zinc. The current of the latter pair was ascertained whilst a part of the ore (perfectly dry) above the surface of the exciting liquid, was touched by the bright end of the conducting wire. This last experiment proves the ore to be a conductor of the current partly produced by its own action, a plate of zinc forming the other voltaic metal. Moreover, when a second piece of ore was placed on the top of the former, and the second piece touched by the end of the wire, the current was still observed.

Having a sufficient quantity of clay to form two partitions, I, on the 1st instant, fitted up another apparatus for the purpose of repeating Mr. Fox's interesting experiments on the transformation of copper ore in the manner described in Vol. I. p. 133, 134, of these Annals. Although rain water alone was the liquid put into the zinc cell, that metal, (a thin piece) had entirely disappeared by dissolution on the 20th day. The zinc plate has been replaced by a new one; and the experiment has not been interrupted one whole day. The copper ore, at present, is partially covered with groups of particles of metallic copper, having the greatest quantity on that side facing the clay and zinc plate. The band of copper wire which surrounds the ore, is likewise covered with particles of precipitated copper. The stratification of the clay ought to be as decisive in this apparatus as in the other.

Another piece of clay is similarly situated in a small gallipot, between a copper and zinc voltaic pair. This apparatus has been in action since the 3d. of February.

W. STURGEON.

Westmoreland Cottage,
May 28th, 1838.

LXXII. *Singular Meteor, seen at Cork.*[*]

To the Editor of the Cork Constitution.

Sir,

On Wednesday evening last, at about a quarter before ten o'clock, as I was coming towards Cork, in a boat, about half-way between the Custom House and the end of the New Wall, I observed a very singular meteor, and as the scientific gentlemen who are engaged on meteorological researches, think it of consequence that accurate public notice should be given of such phenomena, perhaps you will think a description of it worth inserting in your Journal.

The day had been squally with frequent showers from the west, but towards evening the wind had lulled, and no trace of the wildness of the day remained except an occasional flash of faint sheet lightning, which, originating in the west, seemed at intervals to overspread the entire horizon. The meteor caught my eye, at the time and place I have already mentioned, as a speck of light near the zenith, dazzling bright, and larger than the largest star, expanding to a sphere larger than the full moon, and very much brighter, at the same time moving pretty rapidly downwards through about 20 degrees in a N. N. E. direction, when it assumed an oval form and emitted a vivid blue light, it then turned in a direction, as nearly as I could judge, due north, still moving downwards, and by the time it had arrived within a few degrees of the horizon (which at that part of the river is high) was of a pear-like form, when it burst into a great number of lesser meteors of a fiery red colour, which rapidly disappeared beyond the Glaumire Hill.

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[*] Communicated for the Annals, by Miss Mary J. Jennings.

I had no means of ascertaining its actual height; but from the smallness of its apparent velocity, when compared with that of two lesser ones, which I afterwards observed near the zenith, crossing a few degrees from south to north, and then disappearing—assuming the actual velocity, to be the same, I conclude that it must have been at a very considerable height. This is however a very vague result from a very imperfect criterion. It may be well to state the height of barometer and thermometer at Cork about the time, viz., 10 o'clock, P. M. Barometer 29·41; thermometer 50.

I am, sir, yours sincerely,
T. O. R.

Cork, September 2nd, 1837.

N. B. A friend has informed me that similar phenomena were observed at Monkstown (within about seven or eight miles of Cork,) on Thursday evening; but none comparable in size or brilliancy to that described above.

LXXIII. *London Electrical Society.*

Saturday, May 5. David Boswell Read, M. D., F. R. S. E., &c. &c. &c. was elected a Member of the Society.

A letter from Mr. Harper, of Oxford, was read, describing some experiments with a voltaic battery.

A description of a new modification of the galvanometer was read by Mr. Iremonger, by which the deflective power of any electric current, however copious, may be so reduced as to become measurable, even by a delicate magnetic needle. In this instrument the currents above and below the needle pass in the same direction, consequently, their deviating effects being opposed, no action will take place on the needle so long as the currents may be equidistant from each other. In order to enable the instrument to be more effective, the lower current is made to pass along a single wire, and the upper along a series of wires. As the most powerful current will of course preponderate, the deflection of the needle will be the resultant of the difference between the two powers, and depending upon the relative distance of the upper and lower wires from the needle; the upper wires are by a simple but ingenious contrivance made moveable, and the deviating power is thereby completely under the control of the experimentalist. Mr. Iremonger proposes to name the instrument the re-acting galvanometer.

The reading of a paper, entitled. *On the connexion between the atomic arrangement and conducting power of bodies. By* Mr. Thomas Pollock, *was commenced.*

Saturday, May 19. The Rev. Mr. Hoole, of Poplar, was elected a Member of the Society.

The reading of Mr. Pollock's paper was resumed and concluded.

The object of the paper is to obtain by means of the theory of vibration, knowledge more accurate and definite respecting the conducting power in bodies. For if, Mr. Pollock says, the theory be true, its investigation ought to lead to a more convenient arrangement of known facts, to the discovery of new ones, and to the deduction of general laws. This he believed it fully capable of doing by affording an insight into the manner of atomic arrangement in bodies so intimately connected with their conducting powers. It will be impossible to convey to our readers, by a brief report, the extraordinary powers of mind and originality of thought displayed in this investigation, or to bring before them the train of reasoning to support, or the known properties of the different bodies treated of in relation to their conducting power, and explained by the theory of the vibration of matter. We can only cursorily glance at the heads of the investigation, and the recapitulation of the conclusions deducible from their consideration. 1st.—The influence of change of temperature upon the atomic arrangement of nitrous acid gas. Also, 2dly, upon that of red oxide of manganese. 3d.—The same phenomena of change attendant upon vibration. 4th.—Light upon bodies indicates the nature of their atomic arrangement. 5th.—The establishment of the fact that a change in atomic arrangement during vibration is synonymous with conducting power in bodies. From the first and second investigations the following fact was deduced:—that the attractive and repulsive forces among the atoms of the two compounds, nitrous acid gas, and red oxide of manganese, vary with the temperature. By the third it was shown that some atoms do undergo change and others do not. For convenience sake, the atoms of the first class were called re-active; those of the second, oscillating. It was shown also that light upon bodies furnishes a test to determine to which class they belong; reactive atoms giving the optical properties to opaque and coloured bodies, such as metals, charcoal, nitrous acid gas, &c. —oscillating atoms giving properties to transparent bodies, such as hydrogen, oxygen, agate, and water. These two kinds of vibration occuring in matter were illustrated by a diagram. The change of atomic arrangement in nitrous acid gas and red oxide of manganese, during the transition from low to high temperature, with their corresponding alterations of colour, is affected exactly in the same manner by vibration; the influence of the expanding stage being identical with that of a low temperature, that of the contracting stage with a high temperature. Various bodies, analogous in composition to nitrous acid gas, undergo change in atomic arrangement during vibration and in colour, similarly as with the application of heat. The vanadiate of the binoxide of vanadium, according to proportions, produces solutions of different colours. Protoxide of lead is red while hot, but has a rich lemon yellow colour when cold. Red oxide of mercury is nearly black while hot, but red when cold. Protochloride of mercury, or calomel, is yellow while warm, but recovers its whiteness in cooling. But similar instances might be considerably extended. M. Mitscherlich, while establishing his theory of Isomorphism, has shown these changes of colour to be connected with change in atomic arrangement. Mr. Pollock believes that not a single instance can be adduced of a body changing colour by the action of heat, without at the same time undergoing a change in atomic arrangement. The fourth investigation called in the assistance of the undulatory theory of light to distinguish the two kinds of atomic arrangement. The action of light upon bodies furnished a clue to unravel to a considerable extent the mystery of their arrangements, and to classify all bodies into two kinds—those which, when exposed to a varying temperature, change in colour, being also at the same time changed in atomic arrangement: and those which, under similar circumstances, do not change in atomic arrangement to any material extent. Light, by undergoing destruction by its own interference during its action upon bodies becomes a test for change in their atomic arrangement. That it is such a test is evident from the knowledge we possess of the constitution of those compounds so affected during change of temperature;

and in the fifth investigation the question whether this law does not apply to simple bodies was answered in the affirmative; and it does more; it discloses that those simple bodies, such as metals and charcoal, which, as tested by their action upon light, are the most re-active in their arrangement, are also the best conductors. But what puts this fact beyond all further question is the circumstance that charcoal, which produces this interference by its action upon light, is a conductor; while diamond which does not, or if at all to a slight extent, is a non-conductor. These bodies chemists say are identical, the only difference being in the "state of aggregation." If they were to substitute atomic arrangement they would approach much more near to the true statement of the fact. Among the fifty-four simple bodies forty-three are distinctly re-active, the forty-two metals and charcoal; three only oscillating, hydrogen, azote, and oxygen; and eight intermediate, boron, phosphorus, selenium, sulphur, chlorine, iodine, bromine, and fluorine.

The atomic arrangement of the intermediate class is too oscillating to allow them to be conductors, and too re-active to allow them to be decomposed.

It is to be hoped that the science of electricity may remove the mystery attending these bodies. Mr. Pollock says, "the more we know of this science the more remains to be known, and the more sensible are we of our ignorance."

Read also an extract of a letter received by the Honorary Secretary, from Mr. Edward Clibborn, of Dublin, describing some electrical experiments, made with plates of thin glass and talc. The author considers the experiment as setting aside the single fluid theory, and establishes the doctrine of the nature and constitution of the electric charge of the Leyden jar, it also enables us to explain the shock caused by the dry pile, what we may thus imitate mechanically.

END OF THE SECOND VOLUME.